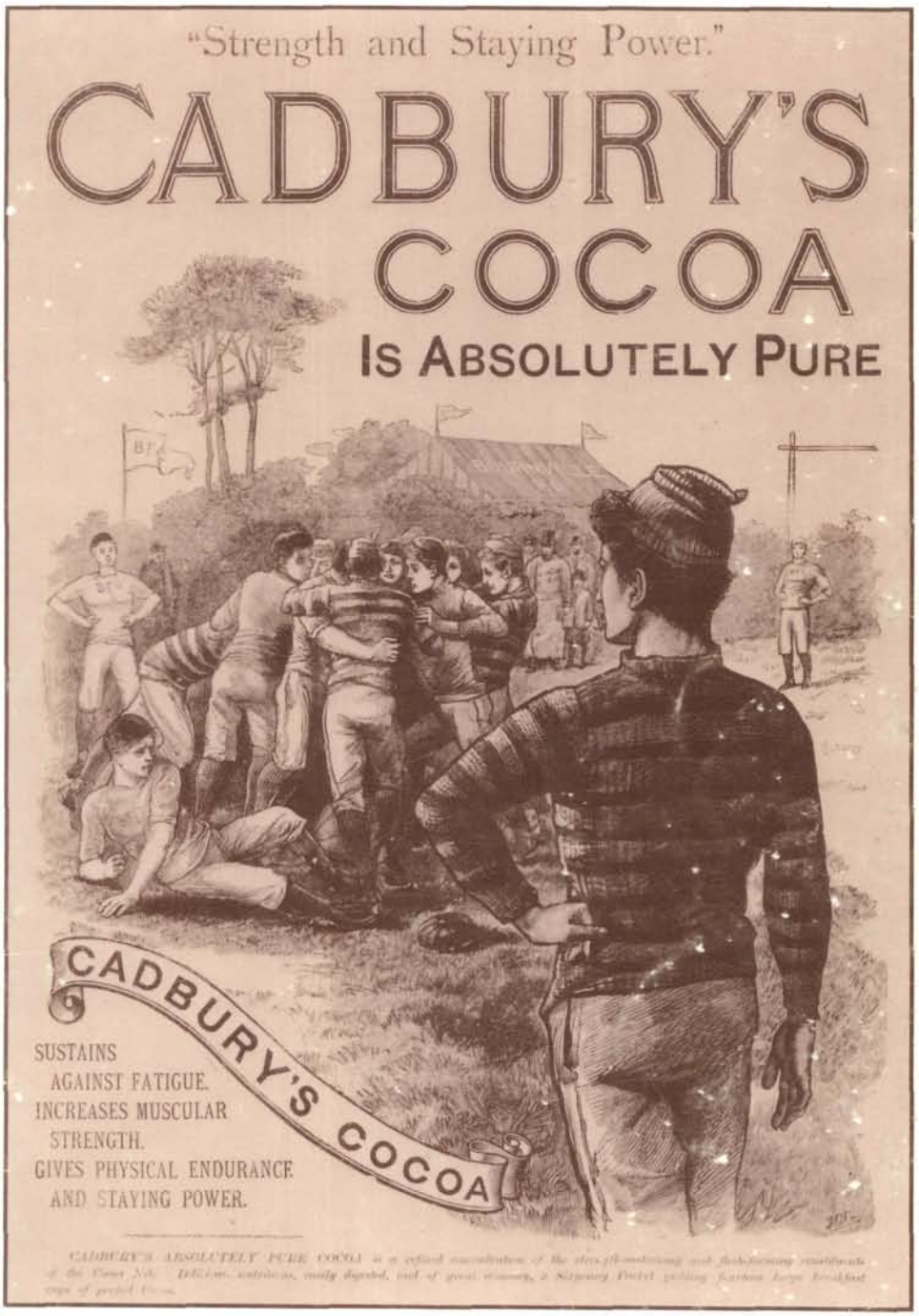
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The American Historical Review

AMERICAN HISTORICAL ASSOCIATION



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The American Historical Review

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In This Issue

This year's presidential address by **Carl N. Degler** proposes a comparative approach to the analysis of American history and American identity. We can best discover what distinguishes us from other nations, he writes, by assessing the divergence of our course from that of other peoples of similar heritage in the areas of political and economic development, social structure, race relations, and religion. This approach, according to Degler, should assist us in overcoming the fragmentation of our historical identity that has been brought on by the recent outpouring of scholarship on groups previously left out of our national history.

Charles Dellheim contributes to the debate on the role of business culture in Britain's industrial decline by examining the business ethos of the *Cadburys* chocolate firm. He argues that the Quaker values of the Cadbury family shaped their business practices and employee policies on the principle that efficiency and worker welfare were interdependent. Though reasonably successful in building cooperation between employees and management by a combination of worker benefits and company rituals of collaboration, these strategies did not fully overcome tensions between economic interest and social ideals, business efficiency and worker welfare.

Robert J. Kaczorowski offers a reassertion of the interpretation of post–Civil War Republican constitutionalism first established in the 1940s and 1950s by Howard Jay Graham, Jacobus tenBroek, and other legal scholars who sought to recover the antislavery origins of the Fourteenth Amendment. Kaczorowski challenges the prevailing view that Reconstruction legislation was limited by the commitment of Republicans to the traditional structures of federalism. He argues that legislators and legal officers of the time interpreted the Thirteenth and Fourteenth Amendments and the Civil Rights Act of 1866 as enactments that revolutionized federal constitutionalism by delegating to the U.S. government primary authority over fundamental rights. The implications of this view for administration of civil and criminal justice by the states, however, caused the Supreme Court to reject the revolutionary theory and restore a traditional notion of federalism.

In an essay that combines American labor history, economic history, and economic theory, **James Livingston** challenges the new economic history and the "organizational synthesis," which imply that modern corporate capitalism was an economic solution to the social impasse of the late nineteenth century. Livingston argues that retardation of the rate of economic growth after 1870 was a consequence of a social stalemate, that renewed growth after 1896 accompanied a shift in the balance of social power, and that the marginalist revolution in economic theory was part of an effort both to understand economic retardation as a problem of distribution and to define capital as a legitimate claimant to a share of the national income.

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In Pursuit of an American History

CARL N. DEGLER

OVER THE LAST TWENTY YEARS, AMERICAN HISTORY has splintered. Indeed, the fragmenting has become so obvious that it is a commonplace in discussions of the state of the American field.[1] The principal source of that disarray has been an explosion of historical information, particularly in social history. The seminal work on the history of slavery in the 1960s stimulated historians to take the same imaginative and probing approaches to the history of cities, blacks, Chicanos, Indians, immigrants, families, and women, and even to transform the history of the economy and politics. Groups and subjects ignored in traditional history suddenly became visible, clamoring for inclusion in a historical framework that once had no place for them. On one level, this informational explosion is what we expect in history, a subject well recognized for changing its content as the society it serves asks new questions and makes new demands. That is the good side of the splintering. On the less appealing side, we have no clear way of determining how this new knowledge will be integrated into what we call the history of the United States. Simply to tack the new information onto the old story disrupts the organizing framework and renders the new version disjointed and incoherent.

Perhaps the most successful and enduring of the framing interpretations for the United States was that created by the "Progressive" historians: Carl L. Becker, Frederick Jackson Turner, Charles A. Beard, and the literary historian Vernon Louis Parrington. The heart of their story was the conflict between democracy and privilege, the poor versus the rich, the farmers against the monopolists, the workers against the corporations, and, at times, the Free-Soilers against the slaveholders. That pattern of interpretation came under attack in the 1950s and has been falling into disuse ever since. Its successor, which has been called the

[1] See, for example, Herbert G. Gutman, "The Missing Synthesis: Whatever Happened to History?" *Nation*, 233 (November 21, 1981): 521, 553–54; "Interview with Herbert Gutman," *Radical History Review*, 27 (1983): 217–22; Olivier Zunz, "The Synthesis of Social Change, Reflections on American Social History," in Zunz, ed., *Reliving the Past: The Worlds of Social History* (Chapel Hill, N.C., 1985), 60, 80; and Thomas Bender, "Wholes and Parts: The Need for Synthesis in American History," *Journal of American History*, 73 (June 1986): 120–36. European historian Peter N. Stearns has suggested that the disarray extends beyond American history. See his "Social History and History: A Progress Report," *Journal of Social History*, 19 (Winter 1985): 319–34. I fully recognize that other peoples in the New World have as good a claim to be known as Americans as the citizens of the United States. I have nevertheless followed throughout this essay the standard practice of referring to the history of the United States as "American" because only that term can describe the people of the United States. No one, so far as I know, has suggested an alternative term, such as "United Statesian."

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"consensus" school of interpretation, had hardly been put into place before it was shot down by a profusion of new research emphasizing the divisions and conflicts in the American past and by actual social and political conflict in the American present of the 1960s and 1970s.

Thanks to the sophistication of the questions we now ask about the American past and the greater care we now take in arriving at answers, we know more today that is significant about the history of the United States than ever before. But the general history we purvey to our students and to the public lacks central themes or a framework. The history of the United States at the present time does not seek to answer any significant questions, such as those the Progressive historians posed when they, perhaps naively but certainly interestingly, asked how "the people" had been treated, or been frustrated, or had triumphed over the special interests. History today, to be sure, no longer seems simple to us; that is a measure of our achievement. But history serves a more vital social purpose than simply documenting the complexities of human behavior, although it certainly ought to do that. If we look to the purposes of history, I think we may discover a framework that not only encompasses the new information but also more effectively achieves those social ends that history alone can fulfill. I am recommending that we begin to shape our presentation of American history around the question, "What does it mean to be an American, that is, a citizen of the United States?" The word "mean" in this context, I hasten to add, should not imply what ought to be or who is a "proper" or "true" American. Rather, I use it in an effort to define us historically. The implicit, but operative, assumption is that Americans differ in some important ways from people of other nations.

The justification for following this line of interpretation rests on two purposes of history. These are not the only uses or purposes, and by advancing them I betray my personal biases. But biases are surely excusable in a profession that no longer expects objectivity. First, history is socially useful. To ask "Who are Americans?" is to raise a question of central importance to citizen and society alike. It is especially relevant to a nation as extensive, as diverse, as recent, and as susceptible to change as ours is. Second, our identity, whether national or individual, comes primarily from history, from the past. The present, after all, is merely a nation's skin, its body is the past.

Although most of you are probably not historians of the United States, virtually all of you are Americans, a fact that, I feel, justifies my pressing this argument on you. As you will recognize as I proceed, what I recommend here as an approach to the organizing of American history can easily be applied to the history of other countries. In any event, my suggestion is not as parochial as it may sound at first.

IF WE TAKE THESE TWO PURPOSES OF HISTORY AS GIVEN, where do we go from there in seeking to describe what an American is? One way is to identify the nature of America today and then to trace through the past the sources of its present identity. I once took that route myself in trying to define how Americans came to be the way they are. The practice has been condemned as Whig history, but it does have

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the advantage of following the sound historical principle of process or continuity. It does little, however, to enlighten us as to what distinguishes us from other peoples. This weakness proved to be nearly lethal to many of the works of American studies that were influential in the 1950s. These studies assumed that, by closely examining some aspect of the American past, historians could identify the traits and values that characterized Americans. Thus articles and books on the American West, the American novel, American pastoralism, and the American cult of violence tumbled from the presses. If the point or outlook that scholars took to be American could be documented from American sources, then the identity of Americans had supposedly been delineated. But, by failing to look beyond the United States, these studies could not distinguish between those traits or developments that were peculiar to Americans and those that other peoples may have experienced.

When one advocates, as I do here, that the United States be compared historically with other societies, the specter of American exceptionalism inevitably floats before us. During much of our history, the idea that America was somehow outside European patterns of development was almost a truism, beginning with Crevecoeur's famous question, "Who is this new man, this American?" and running through Goethe's "Amerika, du hast es besser" to Tocqueville's conclusion that Europe should look to America for a picture of its own future.[2] More recently, starting with Marxists in the early twentieth century, writers have increasingly abandoned the idea that America was an exception to European patterns of development, partly because exceptionalism called into question, at least for Marxists, the idea that there were universal laws of historical development, and partly because it encouraged American chauvinism. Marxists, to be sure, have not been alone in decrying American exceptionalism, especially during the Great Depression, when it seemed obvious that the United States was hardly an exception among the company of industrial nations of European heritage. As one recent critic of American exceptionalism remembered, such an interpretation of American history "made little sense" to him as a junior in college in the 1940s, since the depression's devastation of his family situation and the outbreak of war effectively belied the image of America as the "land of peace and economic plenty."[3] The eclipse of exceptionalism was nowhere more clearly measured than in the assault in the scholarly literature during the 1940s on Frederick Jackson Turner's frontier thesis, which, of course, was an application of exceptionalism.

After World War II and during the cold war years, the idea of American uniqueness was reborn in the form of the concept of "national character," also of

[2] Tocqueville apparently believed that the term "exceptional" would apply to the United States even when compared with later societies: "The position of the Americans is . . . quite exceptional, and it may be believed that no democratic people will ever be placed in a similar one"; Alexis de Tocqueville, *Democracy in America*, Phillips Bradley, ed., 2 vols. (New York, 1948), 2: 36–37.

[3] David W. Noble, *The End of American History: Democracy, Capitalism, and the Metaphor of Two Worlds in Anglo-American Historical Writing, 1880–1980* (Minneapolis, Minn., 1985), 141. Interestingly enough, Daniel Bell's effort to declare an end to exceptionalism did not quite succeed when he considered the American political system. See his "The End of American Exceptionalism," *Public Interest*, 41 (Fall 1975): 222–24.

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ancient vintage. During the 1930s and early 1940s, the assumption that it was legitimate to refer to the character of a whole people had come in for sharp criticism, principally because of the insistence by the German Nazis of the superiority of the so-called Aryan peoples and of Germans in particular. Although the idea of national character had been widely and freely, if loosely, used through a substantial part of the nineteenth century, it began to smack of extreme nationalism, racism, and German Nazism. The work that brought about a reversal in attitude toward the concept was David M. Potter's *People of Plenty*, published in 1954.

Early in his book, Potter spelled out why the idea of national character arose. "The history of American events would be devoid of intellectual challenge if it were merely a literal recording of any events that chanced to occur within American territorial limits. The purpose of history," he emphasized, "is not simply to show that events which might have happened to anyone did happen to someone, but rather to explain why a special sequence of events befell a particular aggregation of people." He went on to say that, to accomplish this goal, the historian must find "what is distinctive in the circumstances, the condition, and the experience of the aggregation in question. But unique circumstances, conditions, and experiences are apt to produce unique traits and attitudes among the people as a whole. To recognize such collective traits and attitudes," he concluded, "is to embrace the concept of national character."[4]

Potter was defending the concept of national character, not exceptionalism—and with good reason. To ask what differentiates one people from another does not mean one has to insist on deviation from a norm, which is clearly implied in the term exceptionalism. In fact, a much sounder approach admits that each nation is unique or exceptional, that there is no general law of historical development, as Marxists implied when they coined "exceptionalism." Recently, for example, German historians have also been debating their version of exceptionalism: the concept of a German *sonderweg*, or "separate way." Like Turner's version of exceptionalism, the German *sonderweg* was once a positive idea, a way of asserting German national superiority. After 1945, the term took on a much less flattering meaning.[5] In itself, the German *sonderweg* reminds us that, in various ways, the history of each nation is unique, which is but another way of recognizing the concept of national character.

Some historians spurn nationally organized history because it is outdated, indeed, moribund, in the face of the pervasive and overpowering forces of

[4] David M. Potter, *People of Plenty: Economic Abundance and the American Character* (Chicago, 1954), 29–30. The sociologist Alex Inkeles has continued to emphasize psychological traits in delineating national character. See his essay, "Continuity and Change in the American National Character," in *The Third Century: America as a Post-Industrial Society*, Seymour Martin Lipset, ed. (Chicago, 1979), 390–416.

[5] For recent literature on the *sonderweg* debate, see Theodore S. Hamerow, "Guilt, Redemption, and Writing German History," *AHR*, 88 (February 1983): 53–72; Robert Moeller, "The Kaiserreich Recast? Continuity and Change in Modern German Historiography," *Journal of Social History*, 17 (Summer 1984): 655–83; David Blackbourn and Geoff Eley, *The Peculiarities of German History: Bourgeois Society and Politics in Nineteenth Century Germany* (Oxford, 1984); and Hans-Ulrich Wehler's review of the German edition of Blackbourn's and Eley's book, in *Merkur*, 35 (May 1981): 478–87.

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technology, urbanization, and industrialization that they perceive as homogenizing the societies of our planet. "The real trend of American history," wrote Laurence Veysey, ". . . is toward a loss of whatever distinctiveness the society once possessed . . . For over a hundred years, and in some respects for much longer, the merger of America into a common pattern of modern life has been the great underlying tendency."[6] On a narrowly practical level, this rejection of national differences seems premature, in view of the difficulties even the advanced industrial nation-states of Europe are experiencing in trying to create a truly common market, not to mention a common parliament. Industrialism may once have been thought to make internationalism inevitable, but, as Ernest Gellner has pointed out, the spread of industrial culture has probably done more to entrench nationalism than any other single force.[7] On a more theoretical level, historians in particular ought to be suspicious of any notion that implicitly denies the role of the past in shaping the present, as Vesey's remarks seem to do. The past as a molder of the present should not be written off just because the forces of modernity seem pervasive. Experience warns us that the past penetrates the present whether we like it or not. No nation escapes its past even when it deliberately seeks to transcend it, as in the ideological revolutions in Russia in 1917 and China in 1948. Nor does a nation necessarily lose its past when foreign conquerors attempt to obliterate it, as the history of Poland reminds us. The ways in which a people adapt to the forces of modern technology or organization are, surely, the product of their history.

BUT, SOME OF YOU WILL UNDOUBTEDLY OBJECT, why should we study how national societies differ from one another? Does not such a procedure overemphasize nationalism and other forces that divide rather than unite peoples? Professional students of nationalism themselves have raised these objections. "I make no secret of my belief," wrote Boyd C. Shafer some years ago, "that nationalism, especially when carried to extremes, leads to war and destruction." Shafer looked forward to the day when it would be shown "that men in every nation are basically more like men in other nations than they are different" and that their "*human* likenesses are possibly much more significant than their *national* differences."[8] It is worth recognizing, nonetheless, as Marc Bloch emphasized half a century ago, that the identification of differences is a primary reason for historical comparison. Through this process, we learn what national events or developments require explanation and how we might explain them.[9] More important, nationalism,

[6] Laurence Veysey, "The Autonomy of American History Reconsidered," *American Quarterly*, 31 (Fall 1979): 477.

[7] "The nationalist principle as such, as distinct from each of its specific forms . . . has very very deep roots in our shared current condition, [and] is not at all contingent, and will not easily be denied"; Ernest Gellner, *Nations and Nationalism* (Oxford, 1983), 118.

[8] Boyd C. Shafer, *Faces of Nationalism: New Realities and Old Myths* (New York, 1972), xiii, 340. Emphases in original.

[9] Marc Bloch, "Toward a Comparative History of European Societies," in *Enterprise and Secular Change: Readings in Economic History*, Frederic C. Lane and Jelle C. Riemersma, eds. (Homewood, Ill., 1953), 507.

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though rightfully charged with many sins, is a deep-seated institution of our time, shaping and vivifying the lives of people throughout the world.[10] We can write history that implicitly denies or ignores the nation-state, but it would be a history that flew in the face of what people who live in a nation-state require and demand. As Potter wrote, the "study of the American people holds little intellectual attraction if the American people are merely an undifferentiated mass of humans fortuitously located in America."[11] We want to know what there is about them that makes them Americans. If we historians fail to provide a nationally defined history, others less critical and less informed will take over the job for us.

A further objection needs to be addressed. Does not the approach I am suggesting ignore conflicts within American society? Was it not this very neglect of internal differences that delivered the *coup de grâce* to the "consensus" interpretation of American history in the 1960s? And has not that lack of recognition of conflict been a legitimate objection to the concept of national character? Conflict or diversity need not be ignored in the approach I am suggesting. On the contrary, any divisions within the nation, by invoking comparison with other countries, would be given a historical measure instead of a subjective one. For example, by comparing cross-nationally the activities of labor unions and radical groups or the extent and character of urban riots over a period of time, we would avoid the ahistorical subjectivity inherent in making judgments about the degree of class consciousness in this country, judgments that are implicitly comparative but which are arrived at from within the experience of the United States. Indeed, studies of social mobility in the United States, on which much scholarly energy has been expended, have suffered from a lack of just this kind of cross-national comparison. Only comparison, after all, can answer the question that prompted the investigations in the first place, namely, was America the land of opportunity that the national myth proclaimed?[12]

It is often said that an emphasis on differences between one national experience and another, such as I am proposing, encourages national hubris. A danger does lurk here but not an inescapable one. We need to recall that the comparative method is used just as often by critics of a given society as it is by those who would extol it. The comparative study of race relations has certainly not resulted in a new sense of pride for Americans. If both critics and champions of a nation use

[10] Gellner, *Nations and Nationalism*, 56, 125.

[11] Potter, *People of Plenty*, 29. Thomas Bender, who also is interested in the creation of a fresh, interpretive synthesis of American history, concluded that national history is not likely to disappear in the near future. "Professional history has been institutionalized on the basis of national cultures as well as states," he wrote. "Anyone with a sense of history must recognize that such associations are contingent, but there is little evidence at the moment of the emergence of any new transnational cultural or political formations to sustain a history that will transcend nations"; Bender, "Wholes and Parts," 125.

[12] One German historian who has a strong interest in the investigation of social mobility has complained that U.S. historians began to lose interest in the question of social mobility when their studies began to reveal more social mobility in the American past than they had anticipated. See Hartmut Kaelble, "Foreword," *Journal of Social History*, 17 (Spring 1984): 406. For a statement on the unfinished state of comparative studies of social mobility, especially between the United States and European societies, see Kaelble, *Historical Research on Social Mobility: Western Europe and the USA in the Nineteenth and Twentieth Centuries* (New York, 1981), 34–57.

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comparison, there is a very good chance the story that emerges will indeed have the critical bite indispensable to any sound national history.

I suggest, then, that we put the history of the United States quite self-consciously, and as consistently as the overall historical account permits, into comparative perspective. This method requires that we raise our eyes from the narrow American scene and ask if what happened here may have differed from what happened elsewhere, and, if so, why? Seeking differences will not overturn the traditional story, for the continuity between past and present—how the past became the present—will remain as pertinent as ever. But comparison will emphasize aspects of our past that may have gone unnoticed before, just as it will call for explanations where none was thought necessary before. The purpose, I emphasize, is not to praise us but to understand who we are. By asking what is American about us, we will also begin to construct a framework that could provide the integrating pattern or synthesis that, at the moment, seems to elude us. In effect, we will be reversing the process that Turner followed when he *assumed* the differences between American and European history and then called on the frontier to account for them. I am suggesting instead that we ascertain what is distinctive about the United States in the surest way we can: by finding out how we have differed from others.

HOW SHOULD THE COMPARISON BE CARRIED OUT? With what countries, for example, should the United States be compared? The comparison should be neither random nor global. Comparisons have traditionally been with Europe, more specifically, Western Europe, for most American immigrants came from that region, and almost from the beginning, Americans have sought, in one way or another, to differentiate themselves from Western Europeans. Comparisons would not necessarily be with all of Western Europe but only with those nations that seem to have shared with us the same historical developments. The purpose of comparison would be to see aspects of our history that differ where we might have expected similarity. For example, the character of American politicians, the nature of our political parties and constitutional practices, the extent of suffrage and popular participation, might be compared with those of England, from which our own political and constitutional practices largely derive. Similarly, our processes of economic growth might be compared with those of Germany, a nation that, like the United States, came late to industrialization.

Some differences in economic development are already known and are suggestive: the absence in Europe of an antitrust movement comparable to that in the United States and the absence in the United States of a socialist movement comparable in strength and influence to those in most Western European nations. This second difference can be further explored by contrasting the fragility and narrow base of the American labor movement over the past century with the experience of organized labor in most industrial states of Western Europe. State-owned economic enterprises are common in Western European economies while almost absent in the United States, another difference in economic life that

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seems worthy of detailed comparative study.[13] Most significant, the possibility exists that, behind these differences (once they are examined together and in detail), we may identify national values that can be legitimately described as "peculiarly American."

European societies are obvious comparisons for this purpose but so are the nations of the New World, for they share with us a European heritage and a novel environment in the western hemisphere. This basis of comparison with the New World has been drawn on before, notably by Latin Americanists such as Herbert Eugene Bolton and Frank Tannenbaum.[14] Most recently, comparisons of slavery in the United States with that in some of the nations of Latin America have yielded valuable insights into the special character of bondage in the United States. Only a beginning has been made in exploring the differences in the reactions of Europeans in the New World to open land, or the frontier. Turner accurately singled out the frontier as a prime source of American identity but for the wrong reasons. He was right, not because the frontier explained us, as he contended, and not simply because its absence from the history of modern Europe differentiated us. He was right because the frontier experience in the United States differed from that in Canada and in Latin America. We know from comparison with Canada, for example, that the long, drawn out, and bloody conquest of native peoples that stained the history of the United States during the nineteenth century had no counterpart across our northern border. When we ask why the difference, we begin to recognize what is distinctively American about our ways of settlement, our forms of frontier government, and our practices of federalism, a recognition that without comparison would have escaped our attention.[15] That in Latin America there are no equivalents of Daniel Boone and Kit Carson and that *frontera* carries none of the meanings and connotations that cluster around "frontier" alerts us again to the different character of the American frontier and the forces that went into the making of Americans.[16] The peculiarly American values that may lie beneath these and other differences can only be uncovered by looking again at the frontier but this time in comparison with the ways other societies confronted open land and native peoples.

The frontier is one situation in which comparison with New World nations would help to identify those elements that went into the making of an American

[13] A table in the *Economist* for December 21, 1985, p. 72, shows that, among seventeen industrial nations, the United States was unique in having a dearth of state-run economic enterprises. Other New World countries such as Mexico, Brazil, and even Canada were conspicuous in their departures from the U.S. pattern.

[14] The explanations are advanced in Mary Floyd Williams, *History of the San Francisco Committee of Vigilance of 1851: A Study of Social Control on the California Frontier in the Days of the Gold Rush* (Berkeley, Calif., 1921), 424–27, and Paul F. Sharp, *Whoop-Up Country: The Canadian-American West, 1865–1885* (Minneapolis, Minn., 1955), chaps. 5 and 12.

[15] It is worth noting that Bolton's use of comparison was quite the opposite of Tannenbaum's (and mine). Bolton was interested in identifying similarities, Tannenbaum in differences.

[16] Alistair Hennessy, *The Frontier in Latin American History* (Albuquerque, N.M., 1978), 11, 158–59. A Briton, Hennessy identified a specific value of North-South comparisons when he pointed out that, if such comparisons were made, "there may be a better chance of getting [British] students to realize that the United States is a foreign country and not just an eccentric version of the British experience"; p. 5.

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nationality. Equally distinguishing is another, the presence of slavery, which set apart the United States not only from Europe but also from those Latin American nations in which slaves were as economically and demographically important as they were in the United States. By bringing Africans to the colonies, slavery left an impress on the social, economic, and cultural history of this country that, even with limited comparison, hints at underlying values and traits unique to Americans. No other society in the western hemisphere in which black people were introduced in bondage equals the record of racism of the United States. No other New World country instituted the social and legal segregation of blacks from whites that, until recently, was endemic here. Although at this point we cannot be sure, it also seems likely that no other New World country counted anything near the almost 2,000 lynchings of blacks recorded in the United States between 1882 and 1902.

YET, AT THE SAME TIME, NO OTHER NEW WORLD COUNTRY has mounted a revolution like the Civil Rights Movement of the 1960s and 1970s in the United States. The contrast is most obvious when the sounds of that upheaval are placed beside the almost total silence on the question of racial prejudice in Brazil, where, even today, only weak and peripheral organizations speak out against the racial discrimination that the social order has long ignored and frequently denied.[17]

More important is the recognition that the Civil Rights revolution was not simply a modern outburst of rage against injustice. Its roots ran deep into the American past. Throughout our national history, the role of black people has been a social issue of import, even though hostility of whites toward blacks has, at the same time, been almost an American trademark. Some white Americans always stood with black Americans and denounced racial hostility as un-American. Again and again in the course of our history, the place of blacks in U.S. society has been a subject of dispute and debate. During and after the American Revolution, the question of race divided Americans. In the middle decades of the nineteenth century, it disrupted the Union and transformed the South and, in the second half of the twentieth century, tore apart the nation's cities and reordered its social agenda. Political leaders from Thomas Jefferson to Lyndon Johnson have had to confront it, while American prophets from Anthony Benezet in the eighteenth century to Martin Luther King, Jr., in the twentieth have used their moral eloquence to keep us from forgetting it. The salience of the issue cannot be explained by force of numbers. All through our history, no more than one American out of eight has been black and often fewer than that. The special place of blacks in the American past is further highlighted when we recognize that no comparable concern was displayed toward the struggle of women for political and social equality. Unlike blacks, women have been left, until very recently, to fight their own battle.

An additional sign of the special role of blacks in the making of America is the distinctive cast that blacks have given to American culture by insisting on being a

[17] See, for example, the recent survey of Brazilian race relations and organizational opposition to racial discrimination in Pierre-Michel Fontaine, ed., *Race, Class and Power in Brazil* (Los Angeles, 1985).

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part of it, as, for example, American Indians have not. Blacks have advanced our economy, inspired our literature, energized our reforms, shaped our music, and redirected our politics. The special place of blacks in our national culture and the accompanying ambivalence of whites toward blacks—that odd combination of appreciation and hatred—are not easy to explain, but, as Gunnar Myrdal contended over forty years ago, they still seem distinctively American.[18]

Deliberate historical comparison would surely throw new light on the meaning of the ethnic diversity that has long been regarded as distinctively American. It is true that England has had its massive Irish immigration and Germany its Polish, but it is the New World countries, with their heavy dependence on European immigration, that offer the most fruitful way of determining if the ethnic past of America is in fact distinctive. We already know that the range of immigrant nationalities and the total number who came to the United States distinguish us from the other nations in the western hemisphere. It is also true that the United States received a substantially greater proportion of European immigrants than any other nation. At the same time, however, the proportion of immigrants in the population of the United States was less than that in several other nations that received immigrants.[19] Yet historians frequently point to the high proportion of foreign-born people as an explanation for the several upsurges of nativism, or opposition to immigrants, in the United States. Comparison with South American societies that received large numbers of immigrants but experienced very little nativist activity might be revealing, not only in regard to the reception accorded foreigners in the United States but also in ascertaining the roots and meaning of American nativism. At least two recent studies, for example, have concluded that opportunities for and acceptance of Italian immigrants were better at the turn of the century in Argentina than in the United States.[20] Comparisons of the social reception and public policies on immigration in the United States and other New World countries would let us ascertain if there was indeed anything that could be termed American about our response to immigrants.

Finally, one difference that seems to identify us but which has rarely been studied comparatively is religion. On the face of it, religion is a distinguishing characteristic of Americans. Today, according to recent public opinion polls, we

[18] Myrdal provided a striking measure of the American concern when he wrote, "Wandering around the stacks of a good American library, one is amazed at the huge amount of printed material on the Negro problem. A really complete bibliography would run up to several hundred thousand titles. Nobody has ever mastered this material exhaustively, and probably nobody ever will. The intellectual energy spent on the Negro problem in America should, if concentrated in a single direction, have moved mountains"; Gunnar Myrdal, *An American Dilemma: The Negro Problem and Modern Democracy*, 2 vols. (New York, 1944), 27.

[19] J. D. Gould, "European Inter-Continental Emigration 1815–1914: Patterns and Causes," *Journal of European Economic History*, 8 (Winter 1979): 604.

[20] Samuel L. Baily, "The Adjustment of Italian Immigrants in Buenos Aires and New York, 1870–1914," *AHR*, 88 (April 1983): 281–305; and Herbert S. Klein, "The Integration of Italian Immigrants into the United States and Argentina: A Comparative Analysis," *AHR*, 88 (April 1983): 306–29. In the absence of any systematic comparative study, an emphasis such as David A. Hollinger placed on the relatively high degree of ethnic acceptance in the United States is questionable. We just do not know that Americans are more accepting than other immigrant societies are until we make the comparisons. Hollinger, "Two Cheers for the Melting Pot," *democracy*, 2 (January 1981): 89–97.

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are the most religious people in the Western world.[21] Religion, usually in the form of Puritanism, has been amply acknowledged in our traditional history. This early Protestantism undoubtedly separates us from any nation to the south of us and from most of those across the Atlantic. I am thinking, however, of more enduring aspects of religious life, those that are still operative, such as the principle of separation of church and state, the wide diversity of sects in the past and present, the voluntarism of Protestant churches, and the relatively recent and massive growth of Catholicism and Judaism. Nor should we forget the founding within our boundaries of two world religions—one frontier and one urban in origin: The Church of Jesus Christ of Latter-day Saints, and the Church of Christ, Scientist. Such obvious differences suggest that cross-national comparisons of public religious policy, church organization, and the place of religion in the social order would tell us much about what it means to be an American.

THE LIMITS OF YOUR PATIENCE AND THE EXTENT OF MY IGNORANCE do not permit me to spell out here other ways we might learn about what it means to be an American, if we would look beyond our borders and shores. Some limited comparative studies in a variety of fields already promise that systematic comparison will provide fresh and revealing ways of defining Americans. We know, to mention four recent examples, that big business came to the fore in the United States long before it did in the first home of the Industrial Revolution,[22] that American wives were much less likely to work outside the home during the nineteenth and early twentieth centuries than were wives in Britain,[23] that public education encountered almost no objections in the United States as compared with the stiff, vocal resistance it met in England,[24] and that reformers during the Progressive era in the United States were much less theoretical than were their

[21] John M. Benson, "The Polls: A Rebirth of Religion?" *Public Opinion Quarterly*, 45 (Winter 1981): 578, 582.

[22] See Alfred B. Chandler and Herman Daems, eds., *Managerial Hierarchies: Comparative Perspectives on the Rise of the Modern Industrial Enterprise* (Cambridge, Mass., 1980), 3, 36–39; and especially the essay by Morton Keller, "Regulation of Large Enterprise: The United States Experience in Comparative Perspective," 161–81.

[23] In his comparative study of Pittsburgh and Birmingham, England, Peter Shergold noted that married women in the British city were four times as likely to work outside the home as wives in Pittsburgh were. Less than 50 percent of unmarried women under the age of twenty in Pittsburgh were employed as against 89 percent in Birmingham. Shergold, *Working-Class Life: The "American Standard" in Comparative Perspective, 1899–1913* (Pittsburgh, Pa., 1982), 74–76. Michael Katz, *People of Hamilton, Canada West: Family and Class in a Mid-Nineteenth-Century City* (Cambridge, Mass., 1975), 273, noted that only 2 percent of married women with husbands present were employed as against 28 percent in Preston, England. Richard J. Evans noted another striking difference between women in America and women in other countries: "The feminist movement in the United States began earlier than elsewhere; and by the end of the nineteenth century, the Americans' domination of international feminism was unchallenged"; Evans, *The Feminists: Women's Emancipation Movements in Europe, America and Australasia, 1840–1920* (London, 1977), 44.

[24] Carl F. Kaestle, "Between the Scylla of Brutal Ignorance and the Charybdis of a Literary Education: Elite Attitudes toward Mass Schooling in Early Industrial England and America," in Lawrence Stone, ed., *Schooling and Society: Studies in the History of Education* (Baltimore, Md., 1976).

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counterparts in Britain.[25] Until we look outside ourselves, we neither know who we are nor what we need to account for in our becoming Americans. The job of comparison is, to be sure, arduous and continuous, though, as Fritz Redlich reminded us some years ago, comparison need not be entirely or mainly archival work.[26] Much secondary literature already exists from which valuable comparisons may be drawn.

If we begin to look at our history as comparatively as we can, it will place us, sometimes with pride, sometimes with embarrassment, and sometimes with shame, in the midst of other nations' histories, clearly identifying who we are. Inasmuch as all nations require a past to obtain the identity history alone can provide, this approach is applicable to any nation. It is, however, especially appropriate to Americans. Our ethnic, racial, and religious diversity help to define us as a people, but diversity, by its centrifugal nature, continually threatens to attentuate, even to dissolve the identity it helped to define. To shape our past around the ways in which we differ from other peoples will assist us in escaping that danger while articulating what it means to be an American. Finally, in pursuing our historical identity, we obtain a framework that can encompass and integrate the new knowledge garnered from the explosion of research in the last two decades. This pursuit will gain for us a history that is distinctively American, not simply because it happened to us, but because it did not happen to others.

[25] Morton Keller, "Anglo-American Politics, 1900–1930, in Anglo-American Perspective: A Case Study in Comparative History," *Comparative Studies in Society and History*, 22 (July 1980): 458–77; and Melvyn Stokes, "American Progressive and the European Left," *Journal of American Studies*, 17 (1983): 3–28. As Ross Evans Paulson's book, *Women's Suffrage and Prohibition: A Comparative Study of Equality and Social Control* (Glenview, Ill., 1973), makes evident, a cross-national study of temperance would give us novel insights into one of the most important aspects of American reform.

[26] Fritz Redlich, "Toward a Comparative Historiography, Background and Problems," *Kyklos*, 11 (1958): 385–87. As Redlich pointed out, some historians have been so committed to archival work that they "have tended to forget that work in archives is not the purpose of historical research but a means toward the end of describing, explaining, and, as I would add, 'understanding' the historical process"; p. 385.

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The Creation of a Company Culture: *Cadburys*, 1861–1931

CHARLES DELLHEIM

THE RELATIONSHIP BETWEEN CULTURAL VALUES AND ECONOMIC BEHAVIOR is a classic theme of historical and sociological literature. Studies ranging from Max Weber's *Protestant Ethic and the Spirit of Capitalism* to Martin J. Wiener's *English Culture and the Decline of the Industrial Spirit 1850–1980* analyze the connections between capitalism and culture.[1] Yet this broad approach has certain limitations. In the search for ideal types, it rarely does justice to cultural diversity. It provides no method to test the impact of social values on economic decisions. And it reveals far more about attitudes to business than about the attitudes of people in business.

Understanding the world views of businesspeople and the cultures of their firms requires more detailed analysis. The current boom in the study of corporate culture and the concern with the role of rituals, symbols, beliefs, and myths within the corporation have suggested new lines of inquiry for business history.[2] Historians of business explore the economic performance and political struggle of firms and industries; entrepreneurship, investments, markets, growth, performance, and succession are among the major themes. It is no criticism of the

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[1] See Max Weber, *The Protestant Ethic and the Spirit of Capitalism* (New York, 1958); Thurman W. Arnold, *The Folklore of Capitalism* (New Haven, Conn., 1937); Benjamin Nelson, *The Idea of Usury: From Tribal Brotherhood to Universal Otherhood* (2d edn., Chicago, 1969); and Martin J. Wiener, *English Culture and the Decline of the Industrial Spirit 1850–1980* (Cambridge, 1981).

[2] See especially Edgar Schein, *Organizational Culture and Leadership* (San Francisco, 1985); Ralph H. Kilmann, *Beyond the Quick Fix: Managing Five Tracks to Organizational Success* (San Francisco, 1984); Thomas Peters and Robert Waterman, *In Search of Excellence* (New York, 1981); William Ouchi, *Theory Z* (San Francisco, 1981); Terence Deal and Allen Kennedy, *Corporate Cultures* (San Francisco, 1982); H. Schwartz and S. M. Davis, "Matching Corporate Culture and Business Strategy," *Organizational Dynamics*, 10 (Summer 1981): 30–48; and Andrew M. Pettigrew, "On Studying Organizational Cultures," *Administrative Science Quarterly*, 24 (1979): 570–81. I set forth a historical approach to organizational cultures in "Business in Time: The Historian and Corporate Culture," *Public Historian*, 8 (Spring 1986): 9–22.

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important work done in this field to suggest that the time has come to supplement studies of strategy and structure with studies of attitudes and meanings.[3]

Recent scholarship suggests that the ethos of a firm, far from being peripheral to its performance, directly influences its success or failure. The dynamics of corporate cultures are, however, rarely appreciated. The historian may make both theoretical and empirical contributions by analyzing how these corporate cultures are created and how they change, or fail to change, in response to social and economic transformations, and by examining the relationship between the individual company and the larger culture and society. A long-term perspective may also clarify the factors that make firms succeed or fail.

This article examines the creation of the company culture of *Cadbury Brothers*, a firm as notable for its enlightened management as for its delicious products. The period explored here begins in 1861, when George and Richard Cadbury took over the family business. It ends in 1931, when capitalist and worker celebrated the firm's values at its centenary. A historical approach to company cultures begins with the guiding beliefs of the founders.[4] Their faith is crucial because they define what A. L. Rice called the "primary task" of an enterprise, the task that it was created to perform.[5] It is necessary to consider how the founders embedded their ethos in the firm and how workers and the public viewed them. The test of a company culture is how principles are enacted and modified in practice. *Cadburys* was not a typical British firm, but it is representative of the Quaker leaven in business and of the movement for "industrial betterment."[6] Thus, it provides an opportunity for analyzing the relationship between religious beliefs and economic action. The history of *Cadburys* contradicts the common assumption that, in business, virtue is its own punishment. The success of *Cadburys* is all the more impressive because the company prospered as Britain declined economically.

THE QUAKER BELIEFS OF THE CADBURY FAMILY SHAPED THE ETHIC OF THE FIRM. The Cadbury family's social and industrial experiments were, on one level, an attempt to reconcile religious convictions and business practices. *Cadburys'* distinctive managerial culture and strategy combined the pursuit of employee welfare with the quest for systematic organization. The Cadburys practiced benevolence without autocracy and pursued efficiency without turning workers into living tools. Three main influences formed George and Richard Cadbury's beliefs: the Quaker ethic, which shaped their views of the nature and purpose of business; the

[3] The seminal work is by Alfred D. Chandler, Jr., *Strategy and Structure: Chapters in the History of American Industrial Enterprise* (Cambridge, Mass., 1962). On the social history of business, see Michael B. Miller, *The Bon Marché: Bourgeois Culture and the Department Store 1869–1920* (Princeton, N.J., 1981), 3–19.

[4] The name of the firm now called *Cadbury Limited* varied somewhat in its own papers. All of the following were used: *Cadbury*, *Cadbury's*, *Cadburys'* and *Cadburys*, which I have chosen to use here. On guiding beliefs, see Stanley Davis, *Managing Corporate Culture* (Cambridge, Mass., 1984).

[5] A. L. Rice, *The Enterprise and Its Environment* (London, 1963), 12–13.

[6] On industrial betterment, see E. H. Phelps Brown, *The Growth of British Industrial Relations* (London, 1959), 76–81.

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experience of turning around a failing firm; and an exposure to the social problems of the industrial city.

The Quaker ethic was the cornerstone of *Cadburys*. During the Industrial Revolution, Quakers achieved unprecedented economic success in ironworking, banking, mining, insurance, and brewing. The Lloyds, the Peases, and the Darbys were all members of the Society of Friends. "The training of friends," George Cadbury observed, "gave them qualities most likely to succeed in business. They were taught self-denial, rigid abstinence from all luxury and self-indulgence."[7] So, too, the Quaker faith sanctioned the pursuit of wealth in a society where wealth was generally a means of social advancement. Barred by their choice of religion from the mainstream of society and the seats of power, Quakers turned their attention to new areas of industry and commerce. Religious persecution and social marginality encouraged entrepreneurship and innovation. As outsiders, they naturally formed close ties among themselves, becoming literally a "society of friends and kinsmen."[8] Their internal solidarity proved an indispensable aid to economic success as the Quaker cousinhood prospered. The Quaker business ethic legitimized but also tempered capitalism by defining the proper means and ends of business. It called for fair dealing and financial rectitude and encouraged the search for "innocent" trades. The stewardship of wealth insisted on the social obligations of riches, but it did not undermine the basis of property interests.

The increase in social acceptance and economic success led to strains in the early nineteenth-century Quaker community. While "plain" Friends such as the Cadburys were content to remain a "peculiar people" set apart by speech and dress, "gay" Friends abandoned the tradition of the simple life. A marked decline of the faith took place among members of leading families such as the Hoares, Barclays, and Gurneys. Wealthy Quakers who stayed in the fold often turned to philanthropy, perhaps to assuage their anxieties about the spiritual consequences of riches. Doing good in the world, preferably in full public view, also brought social status within the holy circle. For these Quakers, philanthropy became a test of religious purity. Despite their support for penal reform, the abolition of slavery, and the temperance movement, some mid-nineteenth-century Quakers were blind to the plight of factory workers.[9] This was not true, however, of the Cadburys.

In the late nineteenth and early twentieth centuries, the Cadburys and the Rowntrees led the quest for industrial reform. Committed to the "unity of life," they applied religious ideals to business. The conviction that every individual must be treated with love and respect militated against viewing labor as a commodity or

[7] Quoted in Elizabeth Isichei, *Victorian Quakerism* (Oxford, 1971), 183.

[8] Isichei, *Victorian Quakerism*, 66.

[9] On Quakers' economic attitudes, see Arthur Raistrick, *Quakers in Science and Industry* (New York, 1950); Paul H. Emden, *Quakers in Commerce* (London, 1940); Frederick B. Tolles, *Meeting House and Counting House: The Quaker Merchants of Colonial Philadelphia 1682–1713* (Chapel Hill, N.C., 1948); Balwart Nevaskar, *Capitalists without Capitalism* (Westport, Conn., 1971); Arnold Lloyd, *Quaker Social History, 1669–1738* (London, 1951); and Alastair Mant, *The Rise and Fall of the British Manager* (London, 1977). There is much of interest in conferences of Quaker employees. See especially *Quakerism and Industry* (Darlington, Eng., 1918). My thanks to Sir Adrian Cadbury for allowing me to read his paper, "Quaker Values in Business," delivered at Gresham College, 22 October 1985.

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a means to an end. The belief in the brotherhood of man—and the spiritual equality of woman—resulted in a hatred of exploitation and a suspicion of the profit motive. It also promoted egalitarian, democratic relationships in the workplace. The Quaker abhorrence of conflict manifested itself in the belief that cooperation was the true basis of business. The custom of "taking the sense of the meeting" also encouraged the search for consensus in the factory. Business was a trust. Workers had the first claim on their employers' benevolence. The goods Quakers produced or sold had to be fairly priced and socially useful.[10] Those who failed to follow these precepts faced the censure of the Society. Sins neglected during private meditations did not escape the scrutiny of watchful brethren.

The milieu in which George Cadbury (1839–1922) and his brother Richard (1835–99) grew up was suffused by the Quaker religion. They traced their Quaker ancestry back through their great-great-grandparents on both sides of the family. Born in industrial Birmingham, they were reared in the affluent suburb of Edgbaston. It was the rural peace and beauty of George's youth that he would attempt to re-create in the village and factory grounds of Bournville. Natural delights were all the more appreciated because the Cadbury household was nothing if not ascetic, disciplined, and severe. No friend of luxury or slave of ease was the father, John Cadbury. There was no doubt as to where he stood in the division between "gay" Friends and "plain" Friends, or, for that matter, where he sat: he refused the comfort of armchairs and allowed no music in his home. A Quaker education strengthened the religious piety of the Cadbury household. Richard was sent to a Friends' boarding school and George to a Friends' day school in Birmingham. Neither boy was exposed to the gentlemanly ethos of the public schools or Oxbridge.[11]

George and Richard Cadbury's mother dubbed them the "Cheeryble Brothers," after the Dickens characters, and, indeed, their kindness and generosity became well known at their firm. Both men were devoted Quakers, though George had little patience for theological niceties or outward forms. Outdoorsmen and sportsmen, they enjoyed playing games with their "workpeople." They did more than talk of fellowship; they performed humble tasks. One worker recalled that "to see Mr. George and Mr. Richard go down on their knees and crawl under the table to see if the water pipes were hot enough made a great impression on all of us."[12] George was a "practical mystic" who perceived no split between the spiritual and material. A passionate social reformer, he had a wide vision but narrow intellectual and aesthetic interests.[13] Natural and unaffected, he put no faith in appearances and mixed easily with his workers. His naïveté and wealth made him a target for greedy hypocrites. Richard, the biography by his daughter tells us, had

[10] John Child, "Quaker Employers and Industrial Relations," *Sociological Review*, n.s., 12 (1964): 293–315.

[11] See A. G. Gardiner, *Life of George Cadbury* (London, 1923), 112–21, and Helen C. Alexander, *Richard Cadbury of Birmingham* (London, 1906).

[12] Cadbury Schweppes Archives, Bournville, Cadbury Brothers Papers (hereafter, CB), Personal Reminiscences: Bridge Street and Bournville, 1929, fol. 164.

[13] Gardiner, *Life of George Cadbury*, chap. 16.

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a sensitive, artistic nature.[14] What is more, he had talent. He was a learned essayist and a skillful painter. Enthusiastic and impulsive, he often lost his temper with employees but was quick to apologize. More traditional in his faith than was George, Richard had a "holy savour about him not easily described, but very distinctly felt."[15]

The experience of turning around a failing business was the second major influence on the guiding beliefs of George and Richard Cadbury. Their father began trading in tea and coffee in Birmingham in 1824; seven years later, he started production on a factory scale. In 1851, Richard joined the business. Although George hoped to become a doctor, his father demurred. George began working at the firm after a brief stint in the Rowntrees' grocery business. When, in 1861, the brothers took over the firm, it was already in rapid decline. The number of employees had dwindled from approximately twenty to eleven, and the firm was losing money. The brothers each invested in the business the four thousand pounds they inherited from their mother. Their capital was nearly exhausted by 1863. Determined not to ask their father for funds or to let creditors lose money on their account, they made plans for George to become a tea planter and Richard a land surveyor. But, in the event, they managed to save the business. It took them ten years to stabilize *Cadburys* completely, but they were showing a modest profit by 1864.

The brothers ensured the success of the firm with two strategic decisions. They changed the basis of the business from tea and coffee to cocoa and chocolate and made dramatic improvements in the quality of their goods. In the early days, George Cadbury remarked, they made a cocoa of which they were not very proud: "only one fifth of it was cocoa, the rest being potato starch, sago, flour, and treacle." The result was a "comforting gruel" much like that of other manufacturers. Not content to make "such extremely common cocoa," *Cadburys* became the first British firm to adopt the Dutch process that pressed the cocoa butter out of cocoa; it gave the cocoa a pleasant taste, eliminating the need for starch and additives.[16] They put "Pure Cocoa Essence" on the market in 1866, popularizing it with an innovative advertising campaign that highlighted its delicious flavor and nutritional value. "Absolutely pure, therefore best" was the slogan. Medical journals such as *The Lancet* and trade journals such as *The Grocer* agreed. The brothers capitalized on this favorable notice, quoting reviews by physicians in their advertisements. Cocoa was a food as well as a drink, suitable for "all classes and all ages."[17] Their efforts were particularly successful because they anticipated, and helped create, the vogue for "pure" foods in England—a vogue given parliamentary sanction by the passage of the Adulteration of Food Acts in 1872 and 1875. In the 1880s, the brothers

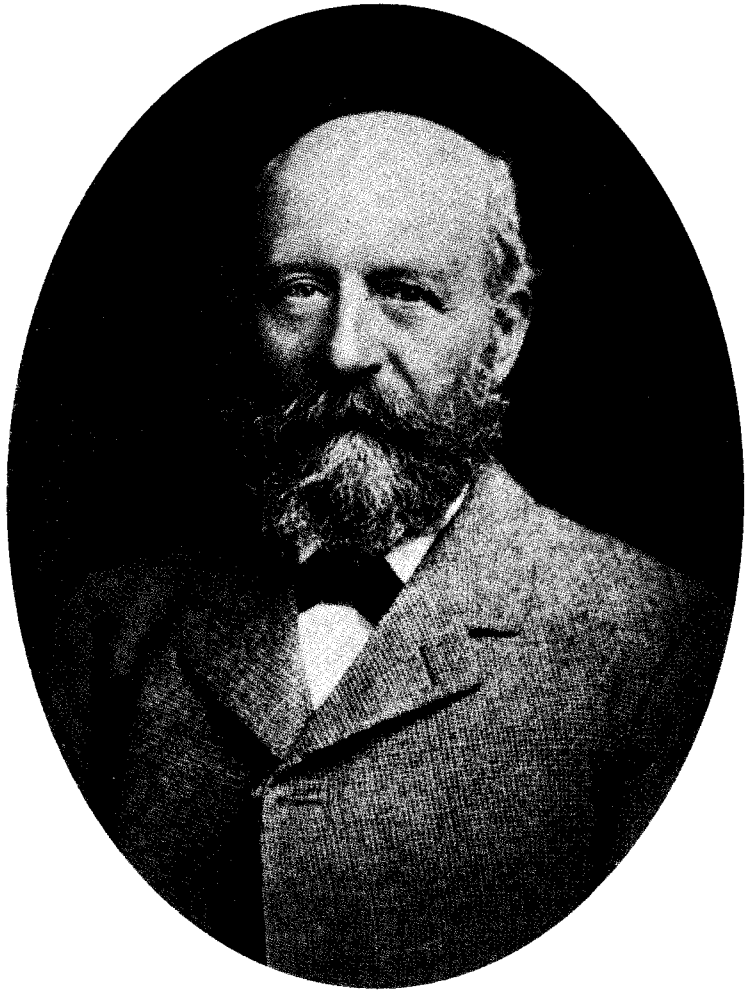
[14] Alexander, *Richard Cadbury*, 118.

[15] Alexander, *Richard Cadbury*, 363.

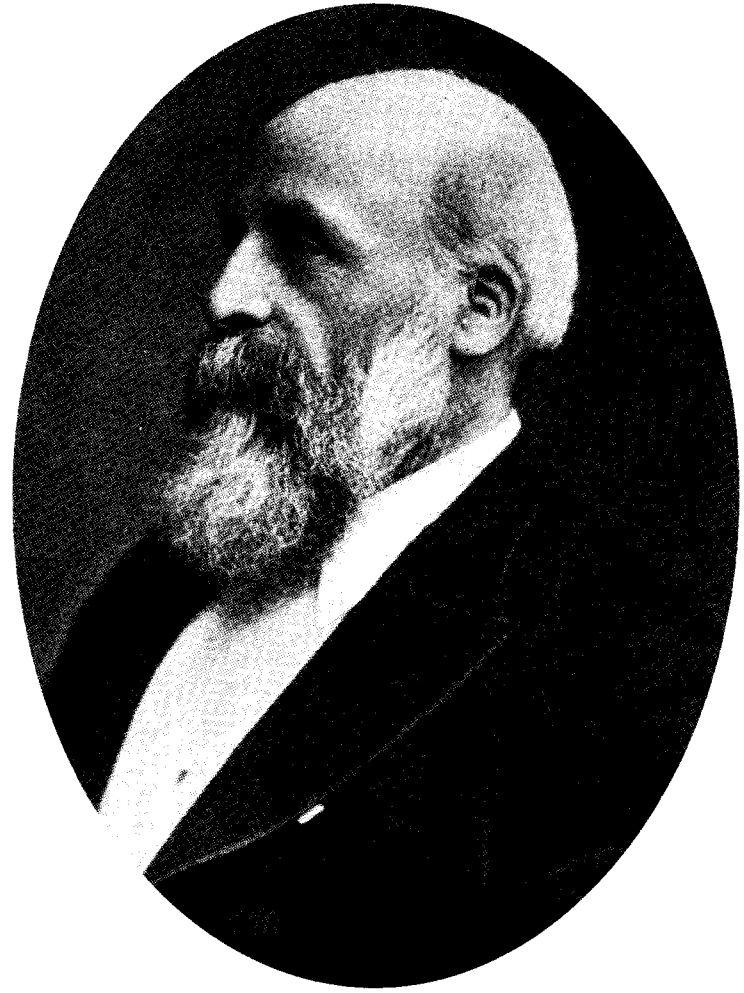
[16] Gardiner, *Life of George Cadbury*, 28. On the general business history of *Cadburys*, see Iolo Williams, *The Firm of Cadbury 1831–1931* (London, 1931), 15–53, and the company's publications such as T. B. Rogers, *A Century of Progress 1831–1931* (Bournville, 1931), or Cadbury Brothers, Ltd., *Industrial Record 1919–1939* (Bournville, 1947).

[17] CB, Bridge Street Book, and Magazine Advertisement Book 1890–1900; Birmingham Public Library (hereafter, BPL), Cadbury Ms. 466/7, fol. 1, *et passim*.

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GEORGE CADBURY.



RICHARD CADBURY.

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turned their attention to chocolate confectionery, creating a series of new lines of plain and filled chocolates. Marketing skill and artistic flair inspired them to decorate their fancy boxes with paintings by Richard, who invented chocolate box art. In 1905, *Cadburys* launched dairy milk chocolate, or "C.D.M.," as it was popularly called. Here too, purity was the key: *Cadburys* used fresh rather than powdered milk. There was a "glass and a half of milk in every half-pound bar" of C.D.M. The name "Cadbury" was written on each square. The other leading product in the years before World War I was Bournville Cocoa, a treated rather than pure cocoa with a stronger chocolate taste than Cocoa Essence.

The struggle to salvage the firm at Bridge Street confirmed the lessons of their Quaker upbringing: the value of hard work and the virtue of personal renunciation. In the commitment to the integrity of their products, the Cadbury brothers sought raw materials of quality and bought much of their raw cocoa in producing regions—Trinidad and the Gold Coast of Africa. Quality meant purity. Cocoa and chocolate suited the Quaker conscience because they were seen as socially beneficial. It was no accident, therefore, that the Cadburys, the Frys, and the Rowntrees dominated these trades. Cocoa and chocolate provided energy and enjoyment at a time when "physical efficiency" was a social concern. And, as a "temperance drink," cocoa was a social good. The crisis of the firm's decline drew George and Richard Cadbury closer to their employees than they might have been otherwise. The Quaker conviction that employers were responsible for the welfare of their employees shaped their conduct. A family atmosphere founded on religion and the personal touch prevailed in the firm. The working day began with a nondenominational morning service. "Mr. George" and "Mr. Richard" called the employees by their Christian names. The traditions of the small industrial workshops of Birmingham bolstered a cooperative spirit, and labor relations were far more harmonious than in the large factories of Manchester.[18] For the Cadburys, the success of the firm depended partly on their personal interest in employees and the cooperation it elicited.[19]

The third major influence on the Cadburys' business ethic was their work in the Adult School Movement of the Society of Friends. The Adult Schools gave young Quakers an important social role denied them within their religious hierarchy. These schools were designed to inculcate moral virtues in poor people, teaching them, as it were, the discreet rigors of the bourgeoisie.[20] George Cadbury began teaching reading, writing, and Scripture in the Severn Street School in central Birmingham in 1859 when he was twenty years old and continued his mission until shortly before his death. Unwilling to restrict his class to "respectable" workers, he sought recruits in the most dismal slums, where he found people faced with hopeless poverty. He organized the classes democratically in order to involve the students in running the school. His aims were to give them self-respect and pride and to help them lead better lives. The Adult School Movement assumed that

[18] Asa Briggs, *History of Birmingham: Borough and City 1865–1938*, 2 vols. (Oxford, 1952), 2: 28–67.
[19] See *Birmingham News*, 28 October 1922.
[20] Isichei, *Victorian Quakerism*, 269–73.

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character could triumph over adversity. "The best way to improve a man's circumstances," George Cadbury noted, "is to raise his ideals." Yet he recognized that it was no less important to transform the environment of the poor. It was not enough to talk to a man about ideals when his home was a slum. "If I had not been brought into contact with the people in my adult class in Birmingham, and found from visiting the poor how difficult it was to lead a good life in a back street, I should probably never have built Bournville village."[21]

In 1878, George and Richard Cadbury decided to build a new factory on a rural site four miles outside of Birmingham. The move to the countryside was unprecedented in business.[22] Their contemporaries generally saw it as an unwise, not to say daft, choice, given the practical difficulties of providing transportation for workers and goods. But the growth of the firm demanded larger premises, and the brothers wanted a site that gave room for expansion. More important, they wanted their employees to have the pleasure of working in the country amid green fields and clean air instead of toiling in the smoky city center of Birmingham.[23] Workers initially commuted from the city to the country; housing was provided for only a few foremen, and workers were reluctant to move to Bournville permanently. Keenly aware of the moral dangers and visual degradation of working-class life in industrial Birmingham, George Cadbury hoped to provide workers with the basis for a better life in the countryside. Bournville was to be an object lesson on the housing problem of the working class. In 1895, he purchased 120 acres near the Works, later buying additional land. Not only did he finance the creation of the suburb from his own funds but he also planned almost every aspect of the village, from houses and roads to parks and trees. Each cottage had a generous yard, or "garden," often with trees and flowers already planted. George Cadbury hoped that doing God's work in the garden would prove more satisfying for workers than doing Satan's bidding in the pub. To play it safe, however, no pub was built in Bournville. The houses were sold or leased to all comers; they were not restricted to the employees of the firm. No one was excluded on the grounds of religious or political belief. In 1900, George Cadbury donated the estate to the Bournville Village Trust, making provision for local self-government. He had no desire to rule Bournville like a latter-day feudal magnate, as William Lever of *Lever Brothers* controlled Port Sunlight.[24]

George and Richard Cadbury ran the firm jointly from 1879 to 1899, with George handling buying and manufacturing, Richard sales and accounts. After Richard's unexpected death in 1899, *Cadburys* became a private, limited liability company with George Cadbury as chairman and his sons Edward and George, Jr.,

[21] BPL, "Mr. George Cadbury at Home," Cadbury Ms. 466/223, fol. 244–45.

[22] William Ashworth, "British Industrial Villages in the Nineteenth Century," *Economic History Review*, 2d ser., 3 (1951): 373–87.

[23] BPL, Richard Cadbury to John Cadbury, 27 September 1879, Cadbury Ms. 466/10.

[24] On Bournville village, see W. Alexander Harvey, *The Model Village and Its Cottages, Bournville* (London, 1906), and George Cadbury, Jr., *Town Planning, with special reference to the Birmingham Scheme* (London, 1918). Initially, the cottages were sold or leased, but after land speculators took advantage of that choice for their own benefit, the cottages were for lease only.

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and nephews Barrow and William as managing directors. Edward took charge of the women's departments and exports. A man of considerable intellectual ability who was ready to take risks, Edward Cadbury worked at a quick pace and held strong convictions that sometimes made him difficult to work with. Shy and sensitive, he feared criticism. Committed to promoting the welfare of the women workers, he was, according to one "forewoman," "a very real friend to whom they could go in any difficulty."[25] His brother, George, Jr., who studied chemistry at University College London, concerned himself with production and distribution. He was responsible for standardizing recipes and for introducing C.D.M. Though careful not to impose his values on others, he was, his son observed, impatient with disagreement—an unfortunate trait in a man who led the Men's Works Council.[26] Barrow Cadbury dealt mainly with finance and accounts but also directed the "Visitors Department." Like his father Richard, he was often impatient and impulsive but was no less ready to apologize. Reserved though he was, he took a personal interest in the workers. "I have always felt," he wrote, "if I was any good in commerce, it was due to the fact that I just didn't experience class distinction."[27] His brother, William, had trained as an engineer, and his main responsibilities were machinery, production, and buying. He encouraged and aided suppliers in Africa to improve their growing methods to ensure the best quality of cocoa. He too was active in public life, serving as a councillor, an alderman, and Lord Mayor of Birmingham. William was an energetic man who appeared grim and austere. Known for his thoroughness, he was also a "great romantic" who would have gone to sea had his father not restrained him. And he was a lover of art and nature, dedicated to the preservation of the Cotswold Hills.[28]

From 1879 to 1931, *Cadburys* grew from a small family firm to the twenty-fourth largest manufacturing company in Britain, worth 10.3 million pounds.[29] To begin with, commercial interests and religious connections shaped *Cadburys*' dealings with its main competitors, *Frys* and *Rowntrees*. Their membership in the Society of Friends made them amicable, respectful rivals, yet rivals, nonetheless. But, by 1900, they had chosen to collaborate on aspects of sales methods, advertising budgets, and prices.[30] The threat of war and foreign competition led them to pool information on costs, machinery, and production processes. In 1919, *Frys* and *Cadburys* merged their financial interests in the *British Cocoa and Chocolate Company*—a move precipitated by the decline of *Frys* and the desire of both firms to expand.[31] *Cadburys* had become the market leader in the pre-war years, surpassing *Frys* in sales. Sales mounted from 1.057 million pounds in 1905 to 2.346

[25] See *Bournville Works Magazine*, 46 (1948): 245–48, and 20 (1922): 57.
[26] *George Cadbury Jr. 1878–1959* (printed for private circulation, 1960), 40–48.
[27] Quoted in Percy W. Bartlett, *Barrow Cadbury: A Memoir* (London, 1960), 33.
[28] *William A. Cadbury 1867–1957* (printed for private circulation, 1958), 15, 41–46.
[29] Leslie Hannah, *The Rise of the Corporate Economy* (London, 1979), 120.
[30] CB, Committee of Management, vol. 1, 19 September 1900, 6 February 1900, and 2 April 1901. *Cadburys*' Board of Directors called itself the Committee of Management until the 1920s.
[31] On the Cheltenham Agreements, see CB, Cadbury Brothers to J. S. Fry and Sons, 6 December 1916; Committee of Management, vol. 18, 17 April 1918, file 387; and Committee of Management Minutes, vol. 7, file 683.

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million in 1914. As chocolate and confectionery accounted for an increasingly large portion of trade, *Cadburys* opened new milk-condensing factories in Knighton (1911) and Frampton (1915). Business doubled during the war, as *Cadburys*, unlike *Frys*, concentrated on "best goods." *Cadburys'* work force also grew dramatically: 3,310 in 1900, 5,730 in 1910, and 7,870 in 1920. The majority of the work force was semi-skilled or unskilled. Women outnumbered men by approximately two to one in 1900, but the ratio was nearly even in 1918.[32] Most women worked in sugar confectionery and paperbox production. The firm reduced costs by employing girls and "boy labour." Trade unions were weak in light industry before the war, and *Cadburys* was no exception. An estimate in 1918 held that only 50 percent of the men and 10 percent of the women at *Cadburys* belonged to unions, although the proportion among skilled workers was higher. There were twenty unions at Bournville, many of which amalgamated in the 1920s.[33]

The decade of the twenties witnessed major changes at the firm. Barrow Cadbury became chairman after George's death in 1922, and Edward succeeded Barrow ten years later. New directors joined the Board after the war: George's son Laurence, and Barrow's children Paul and Dorothy, as well as Walter Barrow and Charles Gillett, who were Cadbury relations. Egbert Cadbury became a director of *Frys*. Laurence Cadbury exemplifies the changing social position of the family: he attended Eton and King's College Cambridge, where he studied economics, and later became a director of the Bank of England. Dorothy Cadbury was one of the few female directors of a major British firm. She began at Bournville as an ordinary pieceworker, originally to further her knowledge of welfare work. As her Uncle Edward had, Dorothy devoted herself to promoting the position of women in industry.[34] *Cadburys* began factory production in Australia, New Zealand, and South Africa because of high import tariffs in these countries.[35] Performance suffered because of the trade depression at home and losses abroad. The Board judged success by sales, and there is no mention of profits in the minutes until the end of the decade.

The unique managerial structure at *Cadburys* depended on a commitment to "associated control," as Edward Cadbury called it, extending from the boardroom to the shop floor.[36] The Board of Directors reached decisions by consensus rather than majority rule. In this, they continued to follow the Quaker custom of "taking the sense of the meeting." The firm had two grades of managerial staff, "Staff A" and "Staff B." There were separate lines of command for men and women in order to increase women's opportunities for advancement. *Cadburys* was divided into specialized departments such as production, sales, buying, and costs, each of which

[32] The figures on sales and workers are culled from CB, Committee of Management, vols. 1900–1930.

[33] CB, Reports in Preparation for the Bournville Works Men's Council 1917–18, 31 October 1917. It is extraordinarily difficult to form an adequate picture of the union experience at *Cadburys* because the only available sources are the records of the Works Councils.

[34] *Bournville Works Magazine*, 51 (1953): 5–7.

[35] See Geoffrey Jones, "Multinational Chocolate: Cadbury Overseas, 1918–39," *Business History*, 36 (1984): 59–76.

[36] *Bournville Works Magazine*, 22 (1924): 73.

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was chaired by a director, who ran it in conjunction with management committees. These committees were the testing grounds of *Cadburys'* team approach. The Men's Works Committee was composed of staff and foremen from sales, chemistry, and engineering. It was responsible for factory conditions, quality control, and welfare work. It followed instructions from the Board—George Cadbury, Jr., chaired the committee—but it devoted more energy to devising improvements than seconding *faits accomplis*. In search of improvements in existing lines and ideas for new goods, the Men's Works Committee tested competitors' products alongside *Cadbury* goods. Careful attention to detail marked their evaluations of flavor, taste, consistency, and presentation. The Men's Works Committee also investigated customer complaints, notably the problem of "foreign matter in chocolate"—unwanted bonuses such as the discovery of a pin.[37] They dealt with complaints promptly, noting their causes and suggesting remedies. Still, the committee system was sometimes slow and cumbersome. But it succeeded in easing friction, building commitment, and creating consensus.

THE INDUSTRIAL EXPERIMENTS AT BOURNVILLE must be seen in the context of a broader concern with social welfare. The "discovery" of poverty in the 1880s and 1890s moved the conscience of the rich. Social investigations such as Charles Booth's *Labour and Life of the People of London* (1892–97) and B. Seebohm Rowntree's *Poverty: A Study of Town Life* (1901) provided detailed statistical information on the conditions of the poor. Churches responded with a social gospel that inspired missions to the poor. The "New Liberalism" advocated state action to remedy social and economic problems.[38] For George Cadbury, man was saved to serve. "The real rest and joy in life," he wrote in 1906, "is to have an assurance that we are filling up the place which God has appointed for us."[39] Quaker idealism inspired his passion for social reform at Bournville and elsewhere. He was an advanced Liberal whose politics owed much to the civic gospel propagated in Birmingham in the 1870s, when he served for several years on the town council. "I am, as you know, a Radical by conviction," he wrote in 1918, "believing that all men are equal in the sight of God, and that every child ought to have a chance, which half the children of this country have not."[40] At Bournville, he gave them a chance, but he also recognized that state action was necessary to effect social reform. The National Insurance Bill, for example, would do little for his own workers, yet it would be "an unspeakable boon to the majority of the workmen who have had no organisation at their back in case of sickness and bad trade."[41] He supported the Liberal party in its efforts to promote the welfare of

[37] CB, Men's Works Committee, Minute Book, no. 1, 10 April 1905, 18 March 1905, 15 April 1905, 13 May 1905, 3 July 1905, and 26 August 1905.

[38] See Asa Briggs, *Social Thought and Social Action* (London, 1961); H. M. Lynd, *England in the 1880s* (New York, 1968); and Peter Clarke, *Lancashire and the New Liberalism* (Cambridge, 1971).

[39] BPL, George Cadbury to Thomas Cooper, 23 October 1906, Cadbury Ms. 466/210.

[40] BPL, George Cadbury to Thomas Cooper, 14 December 1918, Cadbury Ms. 466/210. On the social setting, see Briggs, *Birmingham*, 67–124.

[41] BPL, George Cadbury to Thomas Cooper, 8 January 1912, Cadbury Ms. 466/210.

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Cadbury women at work on production of confectionery, circa 1890.

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the poor. Although he was not a socialist, he was attracted to the cause of the Labour party and enthusiastically supported the alliance between the two parties. He advocated national legislation for old-age pensions and for unemployment insurance, regulation of sweated trades, and compulsory arbitration in trade disputes; the needs of the young and the aged were his special concern.

Quaker religion and Liberal politics also shaped the outlook of George's and Richard's sons. These younger directors were active members of the Society of Friends. Their social concerns—town planning, international peace, women workers, sweated labor, and adult schools—all reflect the influence of their fathers. If they institutionalized their fathers' ideals, they also extended them by industrial experiments. The driving force behind the second generation was the joint pursuit of business efficiency and industrial reform. The social investigations of Edward Cadbury into women's work and sweated labor directly affected his business policy.[42] The sensitivity to the plight of women workers that he acquired in researching *Women's Work and Wages* (1906) stood him in good stead at Bournville. An early member of the Fabian Society, he drew on its work, and on that of Booth and Seebohm Rowntree, for his empirical social investigations. Following his father's example, he ventured into the slums, but, unlike George Cadbury, Edward used a systematic rather than impressionistic approach. In *Sweating* (1907), he described the impact of low wages: "When the labourer cannot maintain himself at a reasonable standard of decency and comfort, the decline in industrial efficiency is rapid. And this is not merely a question of physical efficiency. All the distinctively human qualities that are implied in hopefulness, freedom, self-respect and social ambition, and which are so valuable a national asset, are deteriorated or lost; and thus we get men and women whose spirits are broken, and who become inefficient casual labourers."[43] Edward did not just moralize about the vices of the poor, he also examined the causes of poverty. To his mind, charity and poor relief only perpetuated demoralization and depression. The antidote was industrial reform and state regulation of working conditions and wages.[44]

The Bournville Works became a laboratory for industrial experimentation. Edward Cadbury's researches revealed the necessity of providing greater security and a living wage for workers. Security meant protection in sickness, old age, and unemployment; a living wage meant enough money to provide adequate food, shelter, and clothing for an average-size family—husband, wife, and three children. This base wage represented the minimal amount necessary for "physical efficiency" rather than mere subsistence. A good employer, however, would pay more than the minimum. On the one hand, Edward maintained that men "needed" a higher wage than women: they had to support a family, and, at *Cadburys*, their work caused greater physical strain or required greater skill. On the

[42] George Cadbury, Jr., also made useful contributions to the cause of social reform. See George Cadbury, Jr., and Tom Bryan, *The Land and the Landless* (London, 1908), and G. Cadbury, Jr., *Town Planning*.

[43] Edward Cadbury and George Shann, *Sweating* (London, 1907), 64.

[44] E. Cadbury and Shann, *Sweating*, 65.

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other hand, Edward Cadbury did not countenance the exploitation of women. Indeed, he advocated equal pay for equal work.[45]

The managerial ethic of the Cadburys arose from the firm's position as a family enterprise. George and Richard Cadbury took the long-term view in running their business. "They weren't looking for immediate profit," one employee wrote, "but were building for the future when their children would reap the advantage."[46] Family control of the firm gave the directors great latitude in financing their industrial and social experiments. George Cadbury was reluctant to sell shares in *Cadburys* for fear that stockholders might reject his social aims. "Absolutely Cadbury, therefore pure" might well have been the slogan of the directors. Even when *Cadburys* became a public company in 1912, the family retained the controlling share.

The mission of *Cadburys* was to provide high-quality products at good value to the consumer, and, as one sales representative wrote in 1912, "the advancement of the social, moral, and physical well-being of all connected with Bournville."[47] Edward Cadbury articulated the critical assumption that made the mission feasible. "The supreme principle has been the belief that business efficiency and the welfare of the employees are but different sides of the same problem."[48] Employee benefits would lead to improved efficiency, which in turn would finance welfare schemes. The directors rejected the pervasive, self-serving notion that morality and profit were mutually exclusive. Welfare schemes were a condition of business success rather than a luxury or an afterthought. Unlike early nineteenth-century paternalists, the Cadburys' social aims were in the mainstream of their business. Treating people well was good management and sound principle. The foundation of the firm was the distinctively Quaker fusion of hard-headedness and soft-heartedness. As practical idealists, the Cadburys recognized that humanitarian sentiments could no more substitute for business efficiency than charitable donations could compensate for industrial exploitation.

The core of the younger Cadburys' thought was their unusual approach to power and authority. In the first place, they rejected the idea that labor was a commodity to be bargained for or a cost to be reckoned with by the capitalist. "The status of a man," George Cadbury, Jr., told the Conference of Quaker Employers in 1918, "must be such that his self-respect is fully maintained, and his relationship with his employer and his fellow-workmen is that of a gentleman and a citizen."[49] Second, their labor strategy was to elicit commitment rather than exercise control. The interests of capitalist and laborer complemented each other. Cooperation was the basis of business. Workers should be led rather than driven, inspired rather than coerced, so that they would "take a positive interest in the welfare of the business, and . . . feel that their work and their personality count, no matter how

[45] E. Cadbury and Shann, *Sweating*, 11–16. Edward Cadbury estimated that a living wage in Birmingham would be 25 shillings per week.
[46] CB, Personal Reminiscences, fol. 34.
[47] *Bournville Works Magazine*, 12 (1912): 239.
[48] Edward Cadbury, *Experiments in Industrial Organization* (London, 1912), xvii.
[49] *Quakerism and Industry* (1918): 73.

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humble the position they occupy."[50] Involvement of the workers would build their self-esteem and improve business efficiency. Finally, the loyalty of the workers should not be sought at the expense of individual autonomy and class solidarity. For Edward Cadbury, "The test of any scheme of factory organization is the extent to which it creates and fosters the atmosphere and spirit of cooperation and good will, without in any sense lessening the loyalty of the worker to his own class and organisations."[51] The directors supported the trade union movement. Unlike neo-feudal employers who hoped to control workers, the Cadburys did not want good wages and benefits to encourage dependency and powerlessness. Far from attempting to co-opt workers with promises of security, they actively encouraged them to join trade unions.

Business efficiency also depended on "scientific management," but *Cadburys* practiced scientific management with a difference. Efficiency meant the elimination of waste and the reduction of costs as well as growth in output. It required systematic organization of the factory. The experimental spirit of *Cadburys* manifested itself in the commitment to research and innovation. The directors accepted the necessity of standardizing tools and equipment, determining the proper tools for specific tasks, training the workers, and ensuring that they were suited to their jobs. Yet the Board of Directors did not regard efficiency as an absolute end. The scientific management of Frederick W. Taylor, a Philadelphia Quaker who turned Unitarian, never took hold at *Cadburys*. Hostile to welfare work and trade unions, Taylor was an authoritarian manager who offered an "incentive wage" to maximize output. "Even if on the productive side," Edward Cadbury objected, "the results are all that the promoters of scientific management claim, there is still the question of the human costs of the economies produced."[52] "Speeding-up" and enforcing a single method of performing a task had harmful effects on workers, increasing physical strain and monotony and undermining trade unionism by negotiating with the individual worker.

The mission of the Cadburys shaped their specific goals. The aim of the company, as Edward Cadbury described it, was "a combination of business efficiency together with an all-round development of the workers as individuals and citizens." Business efficiency was necessary to provide "the best possible quality" for the public and a "fair profit" for the firm and the trade. This "all-round development" of the worker required good wages, job security, and a pleasant environment. The Bournville Works, then, had to be a happy place and an aesthetic asset to the community.[53]

THE CADBURY FAMILY CERTAINLY POSSESSED HIGH PRINCIPLES. But high principles alone do not a company culture make; they must be embedded in the hearts and

[50] See Edward Cadbury, "Some Principles of Industrial Organisation. The Case for and against Scientific Management," *Sociological Review*, 7 (1914): 107.

[51] E. Cadbury, *Experiments*, xvii.

[52] See Edward Cadbury's interesting critique of Taylorism, "Some Principles of Industrial Organisation," especially 101–06.

[53] E. Cadbury, *Experiments*, 1; *Bournville Works Magazine*, 22 (1924): 73–76.

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Bournville Football team in front of Bournville Pavillion.

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minds of employees and woven into the fabric of the organization. Capturing the imagination and earning the commitment of workers depended on the management of meaning as much as the organization of routine. Embedding the Cadburys' ethic in the firm required social and industrial experiments in reform, most of which took place from 1902 to 1918. These experiments were largely the work of the younger directors, especially Edward Cadbury. The initiative for the Bournville experiments, and those undertaken by the Rowntrees at York and Lever at Port Sunlight, came, for the most part, from business rather than the state. Legislation followed the practices of enlightened capitalists.[54] The commitment to providing a living wage and job security shaped *Cadbury* policy. The firm paid good wages (supplemented in the 1920s by profit sharing), often on a piecework basis for factory workers. Piecework, of course, was unpopular with trade unions, but Edward Cadbury defended it on the grounds that it enabled workers to earn more than with timework.[55] The work week was progressive by contemporary standards: 42.5 hours for women and 48 hours for men in 1911, 44 hours for men in 1919.[56] *Cadburys* minimized the unemployment and "short time" that seemed endemic to their seasonal trade. It instituted the Benefit Scheme for Sick Employees (1903), the Men's Pension Scheme (1906), and the Women's Pension Scheme (1911). *Cadburys* also offered free medical and dental care at the Works, the dental care being especially useful in a chocolate factory.[57]

Cadburys fostered the "all-round development" of workers by extensive educational programs. After completing a compulsory academic course, workers could pursue commercial or technical training. The firm provided physical education and sports facilities. Bournville, in fact, became a center for social life. Camaraderie flourished in various athletic and cultural clubs. Characteristic of the Cadburys' commitment to social work was the Women Workers' Social Service League, dedicated to improving the condition of women workers in neighboring factories.[58] *Cadburys* gave workers an active voice in the firm. The spirit of involvement and cooperation resulted in the Suggestion Scheme (1902), the Cardbox Shop Committee (1912), and, ultimately, the Works Councils (1918), which were composed of equal numbers of managers and workers.

The Cadburys also embedded their ethos by ritual, myth, and symbol. "The culture of the company," Patrick Joyce observed of the late nineteenth-century factory, "was to be the culture of the employer family."[59] *Cadburys* was a family firm in style as well as name. Family news—births, marriages, and deaths—was dutifully reported in the *Bournville Works Magazine*, which was founded in 1902 to promote the "Bournville spirit."[60] "Mr. George," "Mr. Richard," and their sons after them

[54] Phelps Brown, *British Industrial Relations*, 76–81.
[55] E. Cadbury, *Experiments*, 140–42, 160.
[56] Phelps Brown, *British Industrial Relations*, 79–80.
[57] E. Cadbury, *Experiments*, 140–58.
[58] E. Cadbury, *Experiments*, 224–42.
[59] Patrick Joyce, *Work, Society and Politics: The Culture of the Factory in Later Victorian England* (Brighton, Eng., 1980), 181.
[60] *Bournville Works Magazine*, 1 (1902): 1.

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were the "centre round which everything moved."[61] The Cadbury family presence was felt in the Works and in the surrounding neighborhood. At the heart of Bournville mythology were the figures of George and Richard Cadbury. The brothers' partnership embodied the cooperative ideal they advocated. George Cadbury preached the Christian life he practiced: sobriety, thrift, and respectability. Addressing young Bournville workers at a party in 1918, he exhorted them to lead pure lives of renunciation and service. But, in the same speech, he said: "I want to see those who create the wealth of the world having a larger share of the wealth they create."[62] *Cadburys* perpetuated the founders as symbols after their deaths. More than 16,000 mourners attended the memorial service held for George Cadbury on the Bournville village green in 1922. A special number of the *Bournville Works Magazine*, distributed to employees, customers, and suppliers, idealized his life and work. In this publication, leading managers praised the qualities that made George Cadbury a model employer, notably his wide vision, sympathy with labor, and personal touch. At the suggestion of the Works Councils, a bust of George Cadbury was commissioned. Founder's Day in Bournville village was also celebrated at the Works.[63]

Rituals unified the Cadburys and their employees. In the early years at Bournville, the atmosphere was decidedly religious, though nondenominational. Social occasions dotted the Bournville calendar: Christmas, New Year's, and summer parties, Suggestion Scheme awards, and visits to the Cadburys' homes. Gift giving celebrated the bonds of employer and employed. When a woman worker married and thus left the firm, "Mr. George" bestowed on her a flower, a Bible, and a small sum of money. The flower and the Bible were appropriate symbols of a man devoted to religious truth and natural beauty. They were visible tokens of the "spiritual partnership" the Cadburys beckoned their employees to join. That the amount of money depended on the age of the employee and the years of service was a characteristic concession to practicality.[64] Great occasions brought more dramatic gestures. The firm celebrated its centenary in 1931 with a gift of £50,000 to its employees—distributed, I might add, in the form of National Savings certificates, which they were encouraged, but not required, to keep—and the gift to Birmingham of an open site for public playgrounds and a hospital extension. Not to be outdone, employees bestowed "counter-gifts" such as the Bournville Rest House commemorating the silver wedding anniversary of Mr. and Mrs. George Cadbury, an appropriate echo of the firm's traditional marriage gift. Gift giving involved public affirmations of loyalty to a common cause rather than fealty to a master.[65]

[61] CB, Personal Reminiscences, fol. 107.
[62] *Bournville Works Magazine*, 16 (1918): 295.
[63] See the special number, "The Life and Work of George Cadbury," *Bournville Works Magazine*, 21 (1923): 14–17, and CB, Bournville Works Men's Council, no. 15, 25 October 1926. The firm also distributed copies of Gardiner's *Life of George Cadbury*.
[64] CB, Committee of Management, vol. 1, 31 July 1899.
[65] For the centenary celebrations, see *Bournville Works Magazine*, 29 (1931): 157–85.

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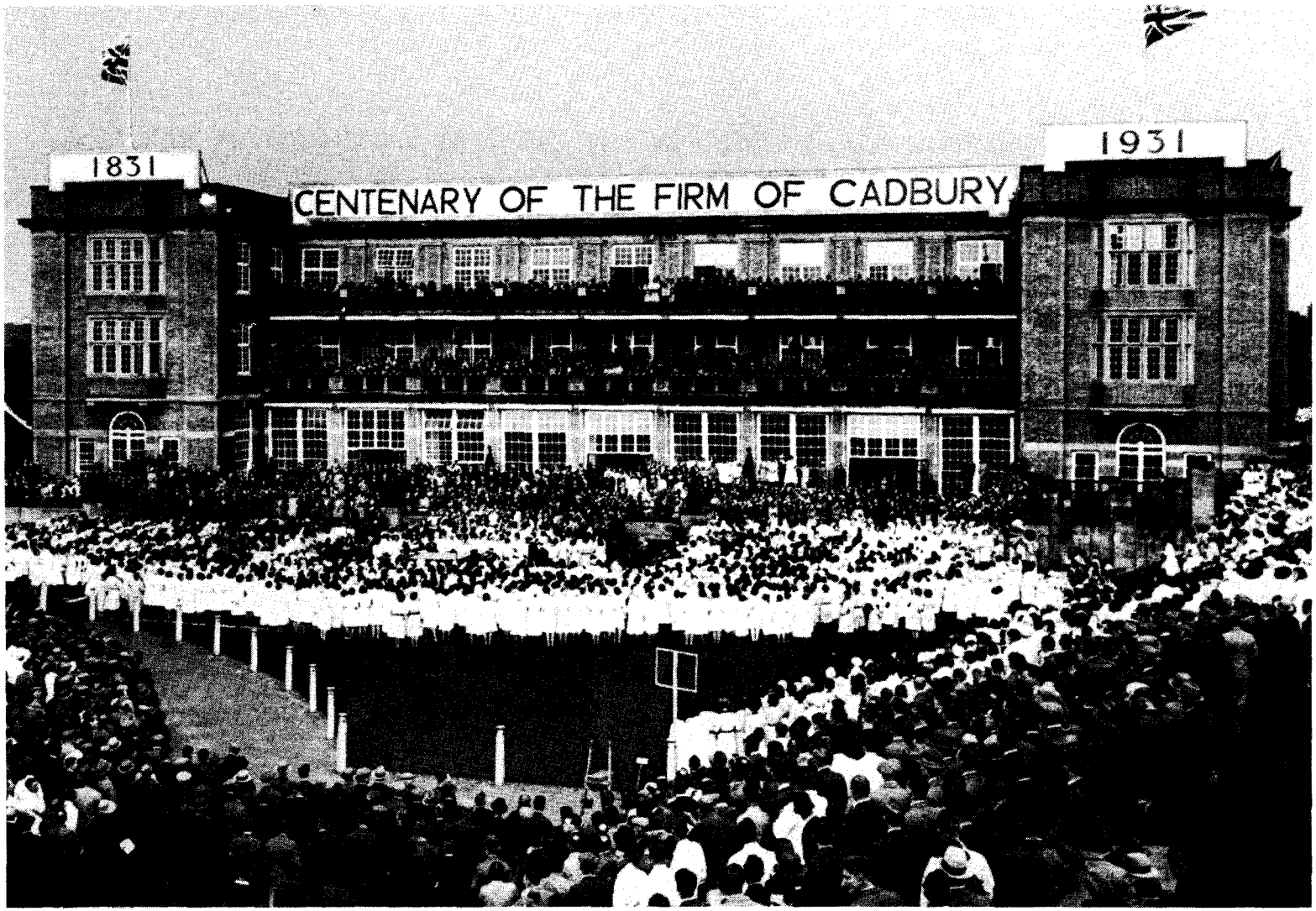
The company saga represented *Cadbury* ideals. Sagas, Burton Clark has argued, turn a place of employment into a beloved institution.[66] They mark two stages in the life of an organization: initiation and fulfillment. The first stage of the *Cadbury* saga was the crisis of the decaying Bridge Street firm. The heroes were the young brothers Cadbury who salvaged the failing company by their persistence and acumen. At the New Year's party of 1913, George Cadbury recalled that "the business was rapidly vanishing . . . It would have been far easier to start a new business than to pull up a decayed one. The prospect seemed a hopeless one, but we were young and full of energy . . . It was splendid training, especially for young men, and I sometimes pity those who have never had to go through it; success is infinitely sweeter after struggle."[67] The moral was the necessity of cooperation: "One cause of the success of the business has been the harmony which has been maintained in the Works. This was the case especially during the first ten years, when the struggle was the hardest, and when the foundations of the present business were laid. Each worker was known by their Christian name, and my brother and I worked beside them."[68] Bournville fulfilled the promise of Bridge Street. The second stage of the saga described the unique accomplishments that made *Cadburys* a distinctive firm. More than just a chocolate and cocoa manufacturer, *Cadburys* was a pioneer of progress, notable for social responsibility, industrial experiments, and the garden village of Bournville. This saga was recited at company rituals, commemorated by a medallion depicting the move from Bridge Street to Bournville, and recorded in histories of the firm and biographies of its founders.

Bournville was the living symbol of the Cadburys' vision of industry. It was captured in a brilliant metaphor, "the factory in a garden," evoking the union of industry and nature. The aesthetic appeal of Bournville was undeniable. Well lit, well ventilated, and clean, the Works were graced by acres of parks and playing fields; everywhere, Bournville boasted greenery and flowers. Even the approach to the railway station was attractive: "Bournville" was tastefully inscribed on a slope above the platform. For George Cadbury, the benefits of country life were immeasurable. "If the Works had been in Birmingham," he told a visiting journalist, "instead of lying at their ease there in the fresh air, with the grass beneath them and the blue sky above them, they would have had nowhere to pass their rest hour except the dingy streets, or perhaps some narrow, evil-smelling court, and they might even have been tempted to spend it in the cheerier surroundings of a drink shop—and who can blame them?"[69] Rural pleasures, then, served moral purposes, enabling workers to lead the good life or, at least, the good life according to George Cadbury.

Projecting *Cadbury* products and identity to the public was also vital for success. *Cadbury* advertising stressed that its products were pure, fresh, and healthy. Avoid

[66] Burton Clark, "The Organizational Saga in Higher Education," *Administrative Science Quarterly*, 17 (1972): 178–84.
[67] See "Life and Work of Cadbury," *Bournville Works Magazine*, 21 (1923): 20.
[68] "Life and Work of Cadbury," 54, 56.
[69] BPL, Cadbury Ms. 466/223, fol. 241.

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The centenary of *Cadburys*, 1931.

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foreign preparations, the customer was advised—"foreign," of course, being a rude word to the English—they were usually adulterated. One advertisement certified the purity of *Cadbury* cocoa by picturing "qualified chemists" at work in a Bournville laboratory.[70] If the aura of science appealed to some consumers, the approval of religion was necessary for others. When a Birmingham rabbi informed *Cadburys* that Cocoa Essence and Bournville Cocoa were suitably pure for the Jewish people, the firm quickly produced a coupon to that effect.[71] By far the most compelling theme used was the superiority of goods made in the "factory in a garden." The logo of *Cadburys* was, appropriately, a tree. Typical of its advertisements was an image of a Bournville recreation ground, with young women lounging by the water or chatting on a park bench.[72] Aware that its social experiments made a favorable impression on customers, *Cadburys* capitalized on the garden village and factory by establishing a "Visitors Department" at Bournville.

To what extent the Cadburys succeeded in communicating their ideals and making Bournville "a happy place" can be judged, first of all, by the memoirs of their employees. These testimonies are scarce, however, and are found largely in "official" records such as the reminiscences collected for the centenary. The accounts contain no traces of the embittered or discontented employees who left Bournville voluntarily or involuntarily. Nor do we hear from those who suffocated in the sweet chocolate atmosphere of Cadburyism. Still, the memoirs illuminate the views and experiences of certain *Cadbury* workers. Their portraits of Bournville life, idealized as they may be, depict a happy, intense place where employees enjoyed their work and social life. Alice Bond came to Bournville in 1885 to work in the Cardbox Department. "Mr. George" impressed her with "his personal interest in everything concerning the workpeople, not only for their physical but for their spiritual welfare." Her feelings for "Mr. Richard" were almost embarrassingly intense, but she buried her erotic impulses in religious imagery. She first saw him at the morning service: "My heart went out to him at once his commanding presence and his beautiful face fit in perfectly with my ideal of what a man of God should be." She and her friends also enjoyed the social life at Bournville: "I remember our first attempt for a summer party, we had a maypole fixed in the small playground, the 24 Juniors looked so sweet in their white dresses and coloured ribbons etc., which the Firm kindly provided . . . such scenes of revelry and fun, in such a perfect setting."[73] Fanny Price, also a pieceworker, cherished Bournville's rural surroundings. When she moved from Bridge Street to Bournville, the brightness and spaciousness of the Works delighted her. "We all loved the country it was such a change from town, and sometimes we would hurry over our dinner in order to go out as soon as we could, to explore the field and lanes."[74]

[70] BPL, Cadbury Ms. 466/12, fol. 27.
[71] CB, Committee of Management, vol. 14, 21 May 1913 and 21 June 1913.
[72] BPL, Cadbury Ms. 466/12, fol. 18–23.
[73] CB, Personal Reminiscences, fol. 6–8.
[74] CB, Personal Reminiscences, fol. 154.

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Bournville men were no less enthusiastic. John Fryer began working in the Moulding Department in 1879, later serving as a foreman from 1894 to 1927. Kindly treated by George and Richard Cadbury at his interview, he reported, "It was all so different from the way the average small boy thought of an employer of labour." Fryer, a sporting man, prized the games and recreation at Bournville. His passion for cricket did not lessen his pride in taking part in the social and educational schemes of the firm. "The roasting Cocoa," he wrote, "means the welfare of the works, and of the district and of the people."[75] It also meant the welfare of William Davenport, who became head of the London office. When Davenport left his job at a Birmingham bank to work at the Bournville factory in 1885, he was struck by the contrast between them. Immediately at home there, he ascribed his good fortune to the "*sense of touch*, with my principals." At *Cadburys*, he found a new spirit in business life. Notwithstanding his own ambition, he was inspired by the Cadburys' personal interest in him and his progress. "They somehow conveyed the idea that their purpose was to do something *for* one, rather than to get the last ounce in the way of work; I think they generally did get the fullest in service, but they *attracted* it—they did not force it." A fine description of leadership. Religion also united William Davenport with his employers. When Davenport left Bournville on his first sales trip, at age twenty-five, George Cadbury told him: "'In all thy ways acknowledge Him, and He shall direct thy "paths." There was nothing parsonic about it, but just something which meant 'that's my experience anyway. You try it for yourself.'"[76]

From the viewpoint of trade unionists, *Cadburys* had the weaknesses of its strengths. H. J. Morcombe, leading craft-unionist and later the first Worker's Secretary of the Works Councils, recognized the disadvantages of benevolence. In 1918, he said of the planned Works Councils, "Now in most factories all these things have to be at the price of fighting, and I often think in a place like this it is difficult to generate interest in things that are really of benefit to the workers in groups because they come to them without quite so much trouble as others may have to put into it in other places."[77] The concern of the directors for the welfare of the workers probably dampened worker militancy. When women workers were asked in 1927 why they stayed at *Cadburys*, one responded: "Because of a feeling of security, also a certain influence from the social and recreational life at Bournville . . . This was possibly good and bad, for it 'nipped ambition in the bud.'"[78] Whether *Cadburys* nipped ambition in the bud is questionable: the firm tended to promote from within.

These perceptions of *Cadburys* provide a glimpse of the bonds that held the firm together. The accounts by employees depict the firm as an instrument for the well-being of all concerned. They demonstrate that "representing a principle" as

[75] CB, Personal Reminiscences, fol. 66, 67, 73, 74.
[76] CB, Personal Reminiscences, fol. 45, 46, 47.
[77] CB, Reports in Preparation for the Bournville Works Men's Council 1917–18, 27 November 1917.
[78] CB, Bournville Works Men's Council, Minutes and Reports, no. 16, January–June 1927.

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well as a business inspired workers to their best efforts.[79] Yet the emotional intensity of these memoirs cannot be explained solely by commitment to abstract principle. From a psychoanalytic perspective, one could argue that what bound the group together was love. One could say that libido was transformed by sublimation into a mythos of fellowship that was socially acceptable and economically productive. The strict separation of men and women and the exclusion of married women strengthened *agape* at the expense of *eros*. Group formation depended on identification with the Cadburys, hence the mystification of their "personal touch." The entry into the firm for most employees at a young age—usually, about fourteen—intensified the bond. It was no accident that they viewed *Cadburys* as a "happy family" or that they idealized "Mr. George" and "Mr. Richard" as kind fathers, supportive friends, or generous benefactors.[80]

The reports of journalists on *Cadburys* were overwhelmingly positive but also idealized: Bournville was a model factory where industrial harmony prevailed. "In these stormy days of strikes and lock-outs," noted the *New Age* in 1898, "when employers and employed seem to wage a ceaseless war against each other, nothing could be more refreshing than to read of a little community where friendship not enmity is the guiding spirit."[81] The garden factory also attracted praise and attention at a time when "the mention of a factory does not usually call to mind green fields and sweetly-scented gardens."[82] The most impressive point was the contribution *Cadburys* made "to the comfort, welfare, and happiness of all who are fortunate enough to be employed permanently by the firm."[83] Late nineteenth-century commentators often misread such efforts as garden-variety philanthropy and charity. Understanding *Cadburys'* attempt to coordinate social and economic aims required a change in public perceptions.

Others held far less flattering opinions of the firm. The attempt to establish the taste for pure cocoa in the 1860s and 1870s outraged manufacturers whose products represented a "perverted taste."[84] The Liberal politics of the Cadbury family, especially George Cadbury's ownership of the "Cocoa Press," antagonized certain conservatives. Especially unpopular was the pacifism of the "fanatical firm of *Cadburys*."[85] Aware that salesmen were often questioned and sometimes harrassed because of the directors' opposition to the Boer War, the directors armed them with Quaker anti-war literature. Even when *Cadburys* finally consented, at Queen Victoria's command no less, to supply plain chocolates to the troops, the firm accepted no profit on the order and refused to place its name on the goods.[86] During World War I, *Cadburys* tried to balance patriotism and pacifism,

[79] CB, Personal Reminiscences, fol. 47.

[80] See especially Sigmund Freud, *Group Psychology and the Analysis of the Ego* (London, 1922), and W. R. Bion, *Experience in Groups* (London, 1960).

[81] *New Age*, 14 October 1897.

[82] BPL, Cadbury Ms. 466/24, fol. 598, 600.

[83] *British Trade Journal*, 1 November 1880.

[84] CB, Bridge Street Book, fol. 97–102.

[85] *Moseley and King's Health Journal*, October 1900, p. 100.

[86] CB, Committee of Management, vol. 1, 14 November 1899, 19 December 1899, and 27 February 1900.

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emphasizing that the question of enlistment was an individual choice. The firm gave allowances to the families of men who did enlist and saved their jobs for them. But the directors would not allow recruiting at Bournville Works or, despite the pleas of Lloyd George, permit the manufacture of wirecutters for the war effort.[87]

Far more damaging to its public image were the accusations of hypocrisy connected with *Cadburys* purchasing raw cocoa grown on the islands of São Thomé and Principe in Portuguese West Africa. The plantations were worked by so-called contract laborers who were, in fact, virtual slaves. "In the plenitude of his solicitude for his fellow creatures," the *Standard* wrote, "Mr. Cadbury might have been expected to take some interest in the owners of those same grimed African hands, whose toil also is so essential to the beneficent and lucrative operations at Bournville."[88] When the Cadburys learned of the plight of these workers, they tried to improve their conditions by bringing "pressure to bear as large buyers of the produce of the colony." After it became apparent that "nothing short of a definite refusal to buy" would have any effect, *Cadburys* discontinued cocoa orders from these islands in 1909.[89] Given the Quaker abhorrence of slavery, one would have expected the Cadburys to have acted quickly and unequivocally. But, although their will was good, their judgment was dubious and their actions were slow. They relied on the advice of the Foreign Office—rarely a wise idea—but even when we take this into account, it is difficult to avoid the conclusion that commercial interests constrained their actions.

IF A COMPANY CULTURE CONSISTS OF A SET OF BELIEFS that are often invoked and rarely enacted, that culture is a façade rather than a foundation. A cynical observer might be tempted to invert the Lancashire saying, "where there's muck, there's brass," assuming instead, "where there's brass, there's muck." The question, then, is how general principles were embodied, and altered, in practice. The crucial testing point was labor policy.

To discover whether *Cadburys* promoted cooperation without lessening the loyalty of workers to their class and organizations, it is helpful to examine the origins and experience of the Works Councils. In 1917, trade-union leader Ernest Bevin approached *Cadburys* and other cocoa, chocolate, and confectionery manufacturers to consider the recommendations of the Reconstruction subcom-

[87] CB, George Cadbury, Jr., to Recruiting Officer, 29th Recruiting Area, 18 April 1915; Lloyd George to George Cadbury, Sr., 6 September 1915; and George Cadbury, Sr., to Lloyd George, 8 September 1915.

[88] *Standard*, 26 September 1908; *John Bull*, 13 June 1908. After the *Standard* article appeared, *Cadburys* sued the newspaper for libel. Although the judge clearly found in their favor, the jury awarded the firm the derisory sum of one farthing. For the general background of the case, see William A. Cadbury, *Labour in Portuguese W. Africa* (London, 1910); Williams, *Firm of Cadbury*, chap. 8; and Gardiner, *Life of George Cadbury*, chap. 13.

[89] For the attitudes and activities of the Board, see CB, Committee of Management, vol. 3, 2 April 1901 and 22 March 1904; Committee of Management, Minute Book 5, statement on Labour in São Thomé, file 436; Committee of Management, vol. 6, statement on Labour in São Thomé, file 713; Committee of Management, vol. 8, 26 November 1907; and vol. 10, 12 January 1909, 9 March 1909.

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mittee chaired by J. H. Whitley, Deputy Speaker of Parliament.[90] The Whitley Report (1917) called for the establishment of national, district, and shop committees representing management and labor, but it was not directly relevant to these industries, which were only partially unionized. The managerial strategy at *Cadburys* was partly a defensive measure designed to avoid government controls. Equally important in promoting industrial democracy, however, was the influence of the Quaker community. During the war, the Society of Friends became disturbed by the parallels between international warfare and industrial conflict. How could they, in good conscience, criticize the one while countenancing the other? Industrial peace assumed new meaning. From 1918 to 1922, Quaker employers faced vehement criticism from their co-religionists, especially from the small band of Quaker socialists. The status and legitimacy of Quaker employers depended on their willingness to participate in the reconstruction of the industrial order. "Foundation of a True Social Order" (1918) rejected "methods of outward domination" in business, calling for cooperation, goodwill, and communal service.[91]

Despite external pressures, the Cadburys greeted industrial democracy with enthusiasm. As usual, they were "eager to be among the first" and "set an example" for other manufacturers. To Edward Cadbury, industrial democracy "was not a new principle for us"; it fulfilled the promise of the Suggestion Committee and the Cardbox Shop Committee.[92] Nevertheless, it was more of a departure than he believed. Although the scheme enjoyed strong support from the managers and workers who helped devise it, the reservations of other workers are telling. One foreman noted that the Works Councils would make his position increasingly difficult. As it was, he was not "top dog" and was unable at Bournville to use the force regularly employed elsewhere. Certain trade unionists feared that the scheme would undermine union power.[93]

Cadburys instituted a three-tier constitutional scheme with Shop Committees representing various trades, Group Committees composed of workers' representatives from the Shop Committees, and Works Councils. Men's and women's committees were separate, but there was also a joint Works Council. Workers

[90] *Cadburys* initially worked out its approach in conjunction with other manufacturers. See CB, Committee of Management, vol. 17a, file 1076. For meetings of the Committee of Cocoa, Chocolate, Confectionary, and Jam Manufacturers, see Committee of Management, Minute Book, no. 18, file 151, 10 January 1918; file 159, 29 January 1918; and file 363, 9 March 1918.

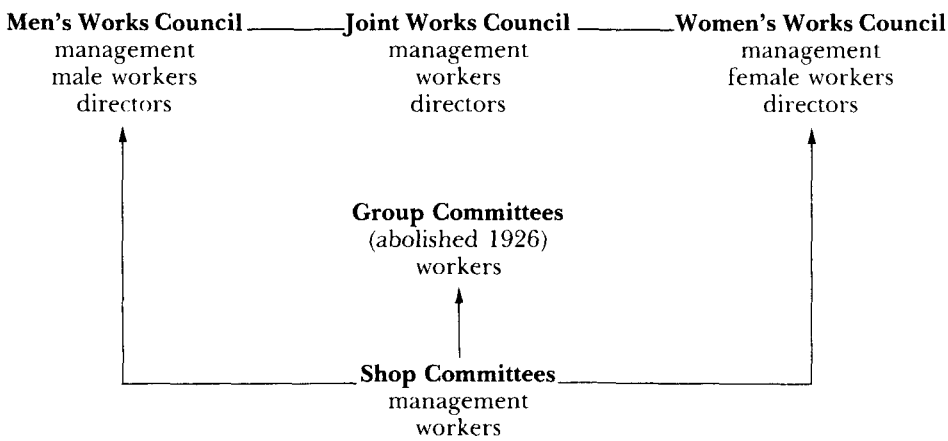
[91] See *Christian Faith and Practice* (London, 1960), sects. 163–69; *The Next Step in Social and Industrial Reconstruction* (London, 1918); *Towards A New Social Order* (London, 1920); and Child, "Quaker Employers."

[92] CB, Reports in Preparation for the Bournville Works Men's Council 1917–18, 31 October 1917 and 27 November 1917. Although the Suggestion Scheme employed direct elections of representatives, the critical Bournville precedent was probably set by the Cardbox Committee. Formed in 1912 by Edward Cadbury in the Women's Department, it originally dealt with piecework questions but soon expanded its scope. The women workers' representatives were elected democratically. There were some problems: first, the inexperience of the representatives; second, the tendency of constituents to blame representatives for unpopular decisions made it difficult at times to find workers willing to serve as representatives. But all concerned agreed that, if the result was not absolute contentment, the committee did contribute to harmony and efficiency.

[93] CB, Reports in Preparation for the Bournville Works Men's Council 1917–18, 27 November 1917.

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ORIGINAL CONSTITUTION OF WORKS COUNCILS (1918)



elected and the Board appointed representatives. George Cadbury, Jr., sat on the Men's Works Council, on the workers' side of the table; Edward and Dorothy Cadbury on the Women's Works Council. The key provision of the scheme was that nothing should be done to contravene union rule or custom; negotiation on wages and conditions was a union preserve. The scope of the Councils included social, welfare, and educational work as well as aspects of industrial production. But the Councils did not make policy. Their purpose was "to encourage and establish good relations between Workers and Management and to maintain a spirit of co-operation, and in this way to promote the welfare and prosperity of the Bournville community."[94] That was no easy task in the 1920s, as recession, labor unrest, and "rationalisation" disrupted dreams of a land fit for heroes. Piecework rates and wages, dismissals and promotions were all subjects of workers' grievances.[95] And deeper questions of good faith and mutual trust arose. Despite the commitment of the directors to frank discussion and open dealing, some workers feared victimization: "If they urge a particular point and cross their Foremen, some day they will find themselves outside the factory."[96] The workers seem to have directed their hostility toward supervisors and managers rather than directors. Politically, this was a useful tactic to gain the Cadburys' sympathy and

[94] CB, Reports in Preparation for the Bournville Works Men's Council 1917–18, 17 April 1918; *Bournville Works Magazine*, 15 (1917): 2.

[95] Piecework rates were a continual source of frustration to management and labor alike. See CB, Pieceworkers Organization Committee, 1920–41.

[96] CB, Workers' Representatives Minutes, Bournville Works Men's Council, 16 February 1923.

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appeal to their sense of fair dealing. Psychologically, it represents "splitting": ascribing evil intentions to the supervisors while idealizing the motives of the Cadburys.[97]

The real test of labor relations came in 1926 with the General Strike. Although *Cadbury* workers insisted that they had no quarrel with the firm, at the height of the disturbances, 44.4 percent of the men, if only 11.7 percent of the women, followed their union's call for a national strike; that number included most of the skilled and many semi-skilled workers.[98] When the strike call came, there was general confusion. Many young women turned for advice to Dorothy Cadbury or their forewomen, who told them "it was a matter they must decide for themselves." Some followed their consciences straight out of the factory gates, explaining that, if they stayed on, they "could not look the others in the face."[99] They returned partly out of fear: they had heard rumors that they would lose their jobs if they stayed out. Some young women said they were happy to be back and were ashamed at what they had done. During the strike, the directors endeavored to keep the Works open. While they decided that the strikers would not be discharged, they pursued a "first out, last back" policy. "All personal feeling and vindictiveness must be ruled out," but the Cadburys wanted to make clear that the "treatment of the firm had not been right."[100] If there had been no quarrel with the firm, why should the firm suffer? After the strike, there was a chill in the air of the factory in a garden. Convinced that the Works Councils had been ineffective during the strike, the Board decided to change their constitution. First, the Board eliminated the trade union qualification for office. This move had scant effect because the commitment to follow union rules and customs continued, and workers still elected union members to the Councils. Second, the Board abolished the Group Committees—on which management had no representation—on the grounds that doing so would permit more direct links between the Shop Committees and Councils.[101] This change provoked the ire of workers' representatives, who protested against what they saw as a retaliatory measure robbing them of the "right of association."[102] But the Board would not relent, presumably because they saw the members of the Group Committees as disruptive militants concerned with promoting conflict rather than fostering harmony.

The Works Councils did succeed, to a point, in establishing good relations. The constitutional system offered workers a forum in which to discuss, and sometimes gain redress of, their grievances and informed them of the state of the business. It provided managers with an opportunity to explain the situations and policies

[97] On "splitting," see Elliott Jaques, "Social Systems as Defense against Persecutory and Depressive Anxiety: A Contribution to the Psycho-Analytical Study of Social Processes," Melanie Klein, *et al.*, *New Directions in Psycho-Analysis* (London, 1955): 478–98.

[98] For the occupational/gender composition of the striking group, see BPL, Cadbury Ms. 466/37, 26 May 1926 and 2 June 1926.

[99] BPL, Cadbury Ms. 466/37, 7 May 1926 and 4 May 1926.

[100] BPL, Cadbury Ms. 466/37, 6 May 1926, 7 May 1926, and 10 May 1926.

[101] CB, Board of Directors, vol. 26, 17 May 1926, and Bournville Works Men's Council, Minutes and Reports, no. 15, Changes in the Organisation of the Men's Works Council.

[102] For workers' responses, see CB, Bournville Works Men's Council, no. 15, 17 May 1926, 7 June 1926, 16 June 1926, and 31 August 1926.

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of the firm. "After all," George Cadbury, Jr., said at a meeting in 1930, "upsets and friction generally come from misunderstanding, and when misunderstanding is cleared away, although there perhaps is still no agreement, both sides understand one another better and know that the other side is trying to do its best. We have the feeling then that we are being fairly treated."[103] For all the tension between the Works Councils and the trade unions, representatives of women workers still held that the two organizations were complementary, even though their relationship needed improvement. The review of the first decade of the Councils concluded: "By those best able to judge, it is generally agreed that the Council has been a great help in facilitating the efficient and harmonious organisation of the factory."[104] But what of those "less able to judge"? Consultative schemes are often more popular with workers' representatives, who enjoy power and prestige, than with their shop floor constituents. After the General Strike, the interest of the workers in the Councils certainly seems to have declined, if temporarily.

Cadburys suffered from a gap between expectations and realities.[105] The very success of the Cadbury family in communicating their vision of industry proved problematic: raising the hopes of workers made the firm's inevitable lapses and shortcomings more disappointing than they would have otherwise been. To some extent, management and workers brought different agendas to the Works Councils. For workers—and this was especially true of the more militant, class-conscious workers who returned from war service—an active voice in the business provided a means to improve wages, conditions, and influence. Although workers made gains in these areas, they were unable to win seats on the major policy-making committees. The directors also faced disappointments. They expected that the Works Councils would contribute to the harmony and efficiency of the firm. The Quaker faith of the Cadburys led them to overestimate the power of goodwill and mutual understanding. If goodwill was indispensable in approaching problems, it did not necessarily solve them. Mutual understanding sometimes aggravated conflict. Insofar as trade unions helped workers become independent and self-respecting, the Cadburys supported them wholeheartedly. But their suspicion mounted when the collectivist approach of the unions led to conflict rather than harmony.

Another crucial principle tested in practice was *Cadburys*' commitment to business efficiency. A focus on efficiency would seem, inevitably, to conflict with the commitment to the welfare of the workers. That the industrial experiments of the firm contributed to its commercial success was, however, an article of faith for the directors. In *Experiments in Industrial Organization* (1912), a study that the firm distributed to staff and supervisors, Edward Cadbury argued that employee programs increased efficiency by eliminating waste and reducing costs. Education improved the performance of workers by enhancing their technical skills and

[103] CB, Bournville Works Men's Council, no. 23, Report of Annual Meeting of Men's Shop Committee, 28 November 1930.

[104] CB, Bournville Works Men's Council, no. 21, Ten-Year Review of the Bournville Works Men's Council 1918–28.

[105] On culture gaps, see Kilmann, *Beyond the Quick Fix*, chap. 4.

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general intelligence. The Suggestion Scheme led to valuable improvements in sales and production. Greater "physical efficiency" increased the alertness and concentration of workers while reducing their rates of sickness and absenteeism. Frequent layoffs would make workers careless and unreliable. Continuing employment served both the firm and the worker. It is important to remember that business efficiency enabled *Cadburys* to provide for its workers. Without careful cost control and stockkeeping, it would have been impossible to reduce unemployment and short time. Efficiency was a moral imperative because it allowed the firm to help its employees; an inefficient business was a social liability.[106]

The course of business efficiency did not always run smoothly. As trade dropped in the late 1920s, *Cadburys* began a program of technological change. *Cadburys* was initially content to carry surplus labor by employing workers in capital development and construction programs and by spreading short time throughout the factory. It became increasingly clear, however, that drastic reductions would be necessary.[107] Workers' representatives first objected to what they saw as departure from past policy but then offered suggestions as to how reductions should be made, trying to gain the best conditions for workers. The firm softened the blow with relief programs and aid to those who wanted to learn a trade or open their own businesses. Here, too, however, there were economic constraints. The Board rejected one hard-hit shop's suggestion to save jobs by reducing the work week from 44 to 40 hours with no loss of wages.[108] The directors ordered layoffs despite the fact that *Cadburys* was still quite profitable. From a commercial point of view, their logic was convincing: increasing output through new machinery, a reduction of staff, and improved marketing would enable the firm to give the best value to the public, which would promote trade and save jobs. But the unwelcome harvest of this policy was the very fear and insecurity Edward Cadbury had hoped to end. The directors took care to mitigate hardship and inform the Works Councils of policy changes, which certainly helped. But knowledge is not always power; sometimes, it is just bad news. By the end of 1930, the worst was over, but more than 2,000 jobs had been lost, largely on the women's side. Output per employee had doubled between 1920 and 1930 and trade had risen considerably. Those who had jobs were more secure; small consolation, of course, to the unemployed.

The final issue concerns whether *Cadburys* encouraged autonomy or dependency. It is conventional to categorize *Cadburys* as a paternalistic firm. George and Richard Cadbury attempted to combine work and family by stressing personal interest in and sympathy with their employees. They endeavored to be both fathers and bosses, protectors and managers. It is true that they sometimes imposed their values on Bournville workers. They would not hire married women, for instance, or permit female workers to continue working after marriage. Their aims were to discourage young women from marrying prematurely and ensure that work did

[106] E. Cadbury, *Experiments*, xviii–xx.

[107] CB, Bournville Works Men's Council, no. 18, Labour Position and Short Time in the Factory, February, 1928.

[108] On the role of the Works Councils, see CB, Bournville Works Men's Council, no. 18, 7 March 1928, 27 April 1928, 17 May 1928; and no. 19, 3 December 1928.

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not interfere with family responsibilities. The Cadburys also feared that allowing married women to work would encourage their husbands to loaf.[109] The result was a discriminatory if well-intentioned policy that robbed women of the right to decide the issue for themselves. The "temperance question," too, left little room for personal choice. In 1900, for example, the Board of Directors decided to give preference to "total abstainers" when hiring salesmen and clerks.[110]

Yet, if paternalism is, as Richard Sennett wrote, "the authority of false love"—false because it keeps the worker inferior and dependent—*Cadburys* was not paternalistic.[111] Indeed, it is more accurate to say that the firm empowered workers by establishing works councils, supporting trade unions, and providing education. *Cadburys* practiced the ethics of social contract, not the ethics of social control. The constitutional structure of the firm militated against the tyranny of personal domination. Although Edward Cadbury was uneasy about compelling young workers to continue their education, he thought the end justified the means: "The class that has education and knowledge is the class that in the long run must rule."[112] He was also concerned by the prospect of *Cadburys* becoming a "greedy institution" that isolated workers from other concerns. But the firm helped workers organize activities for themselves instead of dictating what they should do. Workers governed and partly financed their own clubs.[113] Had George Cadbury wanted to dominate or control his workers, he would have been well advised to turn Bournville into a company town instead of donating it to a trust.

Cadburys experienced the tension between encouraging worker autonomy and maintaining loyalty to the firm, and between spurring worker initiative and providing basic security. Offering security while urging autonomy may create a psychological double bind. Taking good care of people may make them less willing or able to take care of themselves. Successful as the Cadburys were in implementing their ideals, the history of the firm also reveals the limitations of their approach to management. Economic interests played a larger role in the firm than the directors admitted. "We, as directors," George Cadbury, Jr., said in 1918, "have never taken the view of working from a point of how much can be made out of it—but have tried to run [it] for the sake of the well-being of our workpeople. Many of us would die tomorrow if we felt we are working at the expense of our workpeople."[114] This sincere if extravagant statement implicitly denies the very real economic benefits that accrued to the Cadburys. The view of their business as a cooperative venture based on mutual goodwill denied the reality of unequal power. Labor problems dampened Edward Cadbury's optimism about the future of industrial democracy. Although he affirmed the importance of giving the worker some voice in industry, he saw "no way in which [the worker] can be given

[109] On employment policies, see E. Cadbury, *Experiments*, 2–18.
[110] CB, Committee of Management, vol. 1, 27 May 1900; vol. 2, 18 September 1901. There were similar preferences in the choice of foremen. See CB, Foremen's Committee, 16 September 1903.
[111] Richard Sennett, *Authority* (New York, 1980), 51.
[112] E. Cadbury, *Experiments*, 269.
[113] E. Cadbury, *Experiments*, 258–61; Lewis Coser, *Greedy Institutions* (New York, 1974).
[114] CB, Reports in Preparation for the Bournville Works Men's Council 1917–18, 31 August 1918.

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any effective control in large scale industry."[115] Instead, the Cadburys focused their attention on the problem of unemployment, pinning their hopes for a solution on management training and industrial efficiency.

THE BROAD HISTORICAL SIGNIFICANCE OF *Cadburys* needs to be seen in the context of the debate on the relationship between cultural values and economic behavior in modern British history. Martin J. Wiener has argued that attitudes to industrialism played a major role in Britain's economic decline. The middle-class acceptance of gentlemanly standards discouraged wholehearted pursuit of entrepreneurship, productivity, and profit.[116] Although this case study of *Cadburys* does not invalidate Wiener's thesis, it does highlight a different facet of British business cultures.

The experience of *Cadburys* has five major implications. To begin with, though the industrial spirit may have been exhausted in the older firms of established industries such as textiles, this was not true throughout the business world. Witness the success of *Cadburys*, *Rowntrees*, *Lever Brothers*, *Marks and Spencer*, *Boots*, and *Harrods*. Success in light industry and retailing was, of course, no substitute for needed innovation in the rising industries of electricity and chemicals. Industrial leadership could not depend on being chocolatemaker to the world. But, at the same time, historians must ask why firms in heavy industry were unwilling or unable to follow *Cadburys*' successful labor strategy. Second, it is misleading to argue that the persistence of pre-modern values necessarily inhibited economic performance. The chasm dividing "traditional" and "modern" institutions and values in classic sociology obscures nearly as much as it reveals. The pre-modern, "household" values of community, loyalty, and responsibility did not disappear as *Cadburys* became a large firm; the master confectioners blended them with such modern values as efficiency, innovation, and autonomy. No simple shift took place from charismatic to legal-rational authority; the constitutional approach did not preclude the personal touch. Third, the much-debated question of the relationship between religion and capitalism requires revision. Invoking the troubled spirit of Max Weber and the Protestant ethic does not explain the commercial triumphs or failures of various religious sects. A study of *Cadburys* demonstrates the impact of religious beliefs on economic action, but it also underscores the necessity of distinguishing between different religious groups. What set Quakers apart from other Dissenters was as least as important as what united them. Fourth, "paternalism" and "social control" are words that ought to give us pause, conceptually speaking. It is hardly an exaggeration to say that these notions are founded on the questionable assumption that every relationship is a form of manipulation and every act an essay in domination. This is not to deny the reality of exploitation, but historians need a new language for social relations that will do justice to the diversity and dynamics of "family" relations at home and at work. Finally, *Cadburys*

[115] *Quakerism and Industry* (1928): 1.

[116] Wiener, *English Culture*, 3–10; Charles Dellheim, "Notes on Industrialism and Culture in Nineteenth-Century Britain," *Notebooks in Cultural Analysis*, 2 (1985): 227–48.

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illustrates the diversity of business cultures in late nineteenth and early twentieth-century Britain. I concede that, if *Cadburys* was a model firm, it was not typical. Nevertheless, we need far more comparative analysis of company cultures in various economic sectors and regions to establish what was representative and what was not. For the moment, this case reveals the limitations of the monolithic view of capitalism conceived by Karl Marx amid the classical harmony of the British Museum.

Industrial experiments are well known for humane intentions and honorable failures. But *Cadburys* succeeded, through the extraordinary entrepreneurship of George and Richard Cadbury. They created excellent products that met a consumer demand for "pure" foods. *Cadburys* was devoted to the "experimental factor," the spirit of innovation in social policies and technical processes. *Cadburys* was run as a business, not a charity. Goodwill went hand in hand with relentless efficiency. Welfare work was "a vital part of factory organization."[117] The firm succeeded because of the belief that "it is the human factor which is of more importance than anything else in industry"—a lesson embodied in the company culture of *Cadburys*.[118]

[117] E. Cadbury, *Experiments*, 262–64.
[118] *Quakerism and Industry* (1918): 74.

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To Begin the Nation Anew: Congress, Citizenship, and Civil Rights after the Civil War

ROBERT J. KACZOROWSKI

IN 1857, THE HIGHEST COURT IN THE UNITED STATES HELD THAT BLACKS in America possessed no rights, could never become citizens of the United States, and that Congress was powerless to abolish slavery.[1] In the aftermath of these pronouncements, this country fought one of the bloodiest wars in its history. Fewer than ten years after the *Dred Scott* decision, however, Congress and the Northern states accomplished precisely what the Supreme Court declared could not be done, through constitutional amendments and a civil rights statute. The Thirteenth Amendment abolished slavery everywhere in the United States. The Civil Rights Act of 1866 and the Fourteenth Amendment conferred citizenship on and secured the civil rights of all qualified, natural-born, and naturalized Americans, including former slaves and free blacks. The statute declared illegal infringements of certain civil rights made under the prextext of law or custom and authorized the removal of civil and criminal cases from the state to the federal courts whenever Americans were unable to enforce their rights in the state systems of justice. The Fourteenth Amendment also expressly prohibited the states from infringing the rights that Americans enjoyed as citizens of the United States and their rights to due process and equal protection of the law.

The meaning and scope of the Thirteenth and Fourteenth Amendments and the Civil Rights Act of 1866 have been almost as controversial among twentieth-century scholars as they were among the participants in Reconstruction. In 1947, Supreme Court Justice Hugo Black sparked a debate over the scope of a national authority to enforce civil rights when he held that the Fourteenth Amendment conferred on the national government the power to protect the Bill of Rights against state infringements.[2] Charles Fairman quickly wrote a rebuttal, in which he insisted that the congressional framers intended to secure only an equality in state law among the few rights enumerated in Section 1 of the Civil Rights Act, a view he continued to maintain. William Crosskey published a rejoinder to Fairman,

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[1] *Dred Scott v. Sanford*, 19 How. (60 U.S.) 393, 404 (1857).

[2] *Adamson v. California*, 322 U.S. 46, 68 (1947) (Black, J., dissenting).

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in which Crosskey asserted his view of a nation-centered federalism that delegated to Congress the authority to enforce the Bill of Rights, a view he later elaborated in his three-volume history of the U.S. Constitution.[3] The specific question whether or not the Fourteenth Amendment incorporates the Bill of Rights has been vigorously debated, most recently by Raoul Berger and Michael Curtis.[4]

Other legal scholars and historians have focused on additional aspects of a national civil rights enforcement authority. Jacobus tenBroek and Howard Jay Graham argued that the theory of the Reconstruction amendments derived from the natural rights ideology of the abolitionists, and that the amendments delegated to Congress an expansive authority to enforce fundamental rights.[5] They assumed that it was the Radicals who controlled Congress during Reconstruction and incorporated their expansive view into the Reconstruction amendments. Revisionist historians in the 1960s argued that the moderate Republicans, not the Radicals, controlled Congress and formulated Reconstruction policy.[6]

Although revisionist political historians of the 1960s agreed that even moderate Republicans were committed to securing civil rights after the Civil War, revisionist legal historians in the 1970s argued that the Reconstruction amendments did not significantly alter American federal constitutionalism. Insisting that the moderates were legal conservatives who consciously intended to avoid a revolutionary restructuring of constitutional law and to retain a federalism based on states' rights, the revisionists concluded that the Civil Rights Act and the Fourteenth Amendment preserved in the states primary authority over citizenship and civil rights. The states therefore continued to enjoy the authority to determine the status of Americans, define their rights, and provide for their protection. The framers of the legislation intended that citizens redress violations of civil rights and enforce their rights through state courts and law enforcement agencies. Conscious of the revolutionary implications for American federalism, the framers conferred on the national government merely the authority to prohibit the states from discrimi-

[3] Charles Fairman, "Does the Fourteenth Amendment Incorporate the Bill of Rights? The Original Understanding," *Stanford Law Review*, 2 (1949): 5; Charles Fairman, *Reconstruction and Reunion, 1864–88* (Part One), vol. 6 of *History of the Supreme Court of the United States*, P. Freund, ed. (New York, 1971), see especially chaps. 19 and 21; William Crosskey, "Charles Fairman, 'Legislative History' and the Constitutional Limitations on State Authority," *University of Chicago Law Review*, 22 (1954): 1; see also Charles Fairman, "A Reply to Professor Crosskey," p. 144; William Crosskey, *Politics and the Constitution in the History of the United States*, 3 vols. (1953, 1980), especially chaps. 30–32.

[4] For the recent debate, see Raoul Berger, *Government by Judiciary: The Transformation of the Fourteenth Amendment* (Cambridge, Mass., 1977); Michael Curtis, "The Bill of Rights as a Limitation on State Authority: A Reply to Professor Berger," *Wake Forest Law Review*, 16 (1980): 45; Michael Curtis, "The Fourteenth Amendment and the Bill of Rights," *Connecticut Law Review*, 44 (1982): 237; Michael Curtis, *No State Shall Abridge: The Fourteenth Amendment and the Bill of Rights* (Durham, N.C., 1986); Raoul Berger, "Incorporation of the Bill of Rights in the Fourteenth Amendment: A Nine-Lived Cat," *Ohio State Law Journal*, 42 (1981): 435; and Raoul Berger, "Incorporation of the Bill of Rights: A Reply to Michael Curtis' Response," *Ohio State Law Journal*, 44 (1983): 1.

[5] Jacobus tenBroek, *Equal under Law* (New York, 1965); Howard Jay Graham, "The Early Antislavery Backgrounds of the Fourteenth Amendment," *Wisconsin Law Review* (1950): 479; and Howard Jay Graham, "Our Declaratory Fourteenth Amendment," *Stanford Law Review*, 7 (1954): 3.

[6] See Eric McKitrick, *Andrew Johnson and Reconstruction* (Chicago, 1960); William Brock, *An American Crisis: Congress and Reconstruction, 1864–1867* (New York, 1963); and La Wanda Cox and John Cox, *Politics, Principle, and Prejudice, 1865–1866* (New York, 1963).

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nating among citizens on the basis of race in matters relating to civil rights and to punish state officers who did so.[7]

CONGRESSIONAL REPUBLICANS BELIEVED THAT THE THIRTEENTH AND FOURTEENTH AMENDMENTS and the Civil Rights Act of 1866 represented a revolutionary change in American constitutionalism.[8] A change in federalism was a prerequisite for Congress to legislate for the protection of civil rights, in light of the nineteenth-century concept of federalism. If the status and fundamental rights of citizenship were the rights that individuals enjoyed as citizens of the states, Congress would not have had the authority to legislate for their protection. These fundamental rights would be within the exclusive jurisdiction of the states. The proposal by Congress of a constitutional amendment and a statute that conferred on all Americans the precious status of citizen, enumerated some of the fundamental rights of citizenship, and extended to citizens federally enforceable guarantees for the protection of their civil rights was itself a revolutionary change in American federalism.

The radical change in American constitutionalism represented by the actions of Congress forced congressional Republicans to formulate a legal theory delegating to Congress the authority to secure the status and civil rights of Americans. Republicans explained that sovereignty resided in the national government and included the primary authority to determine the status and secure the rights of all Americans, white as well as black.[9] They interpreted the Thirteenth Amendment

[7] See Fairman, *Reconstruction and Reunion, 1864–88*, 1228–29; Berger, *Government by Judiciary*; Harold Hyman, *A More Perfect Union: The Impact of the Civil War and Reconstruction on the Constitution* (New York, 1973), especially pp. 457, 460–68; Michael Les Benedict, *A Compromise of Principle: Congressional Republicans and Reconstruction* (New York, 1974), 27, 41, 48, 56–69, 122–26, 147–49, 170; Michael Les Benedict, "Preserving the Constitution: The Conservative Basis of Radical Reconstruction," *Journal of American History*, 61 (1974): 65; Phillip Paludan, *A Covenant with Death: The Constitution, Law, and Equality in the Civil War Era* (Urbana, Ill., 1975), 58, 261, 274–75; Herman Belz, *Emancipation and Equal Rights: Politics and Constitutionalism in the Civil War Era* (New York, 1978), 108–40; and Herman Belz, *A New Birth of Freedom: The Republican Party and Freedmen's Rights, 1861–1866* (New York, 1976), 157–77.

[8] See Robert J. Kaczorowski, *The Politics of Judicial Interpretation: The Federal Courts, Department of Justice and Civil Rights, 1866–1876* (New York, 1985), and "Revolutionary Constitutionalism in the Era of the Civil War and Reconstruction," *New York University Law Review*, 61 (1986), forthcoming. The essential conceptual framework and conclusions of this study relating to the legislative history of the Fourteenth Amendment and Civil Rights Act of 1866 were originally presented in Robert J. Kaczorowski, "The Nationalization of Civil Rights: Constitutional Theory and Practice in a Racist Society, 1866–1883 (Ph.D. dissertation, University of Minnesota, 1971); "Race, Law and Politics: Congress and Civil Rights after the Civil War," paper presented at the Annual Meeting of the Southern Historical Association, November, 1973; and "Civil Rights in the Lower Federal and State Courts during Reconstruction," paper presented at the Annual Meeting of the Organization of American Historians, April, 1975. See also Harold Hyman and William Wiecek, *Equal Justice under the Law: Constitutional Development, 1835 to 1875* (New York, 1982), 386–438; Belz, *Emancipation and Equal Rights*, 108–40; and Belz, *A New Birth of Freedom*, 157–77.

[9] For example, the "father" of the Fourteenth Amendment, Cong. John A. Bingham of Ohio, in a statement that represented the views of his congressional colleagues, declared that the authority to secure the rights of citizens "belongs to every sovereign power, and is essentially a subject of national jurisdiction." *Congressional Globe*, 39th Cong., 1st sess., 1866, p. 1090. This view of national sovereignty was also expressed outside of Congress. See the letter of "Madison" published in the *New York Times* (November 15, 1866): 2. Other scholars have noted this interpretation, but they reached different conclusions concerning the scope of authority the amendment delegated to Congress. See Belz,

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as a constitutional guarantee of the status of Americans as free people and therefore as a delegation of authority to Congress to secure the fundamental rights of American citizens. Congressional Republicans reasoned that the amendment, in abolishing slavery, secured liberty and the rights of free people. They equated the status and rights of free people with the status and natural rights of citizens. Congressional Republicans understood the Thirteenth Amendment as a guarantee of the status and rights of citizenship. Applying a Hamiltonian, nationalistic interpretation of the Constitution, which attributed to Congress the authority to secure rights that are recognized or guaranteed by the Constitution, they concluded that the Thirteenth Amendment delegated to Congress the authority to prohibit slavery and, more important, the authority to secure inherent rights of all U.S. citizens against violation from any source in whatever manner Congress deemed appropriate.[10] Thus, James F. Wilson, the representative from Iowa and the House floor manager of the Civil Rights Bill, introduced it with the explanation "that the possession of these rights by the citizen raises by necessary implication the power in Congress to protect them."[11]

Republicans expressed in law their understanding of the scope of the Thirteenth Amendment when they enacted the Civil Rights Act of 1866 and the Fourteenth Amendment. Section 1 of the Civil Rights Act confers citizenship on all qualified American inhabitants and guarantees to all American citizens at least some of the rights the framers believed to be fundamental.[12] They added a similar citizenship clause to the first section of the Fourteenth Amendment in the event that a subsequent Congress repealed the Civil Rights Act. The addition of this clause was also designed to prevent courts from declaring the statute unconstitutional by

Emancipation and Equal Rights, 108–40; Daniel Farber and John Meunch, "The Ideological Origins of the Fourteenth Amendment," *Constitutional Commentary*, 1 (1984): 235, 262–63.

[10] The framers believed that the Congress possessed plenary authority over civil rights, as established by the following: *Congressional Globe*, 39th Cong., 1st sess., 1866, pp. 41–42 (Sen. Sherman); pp. 43, 474, 476, 527–28, 573–74, 600, 1758, 1780–81 (Sen. Trumbull); pp. 503–04 (Sen. Howard); p. 570 (Sen. Morrill); pp. 602, 741 (Sen. Lane of Indiana); p. 703 (Sen. Fessendon); p. 768 (Sen. Johnson); pp. 571, 3031–32 (Sen. Henderson); p. 1225 (Sen. Wilson); pp. 1255, 1780, 3037, Appendix 101 (Sen. Yates); Appendix 96 (Sen. Williams); pp. 1033, 1088–94, 1291, 2542 (Cong. Bingham); pp. 1115, 1118–19, 1294–95 (Cong. Wilson); p. 1120 (Cong. Rogers); p. 1124 (Cong. Cook); pp. 1151–53 (Cong. Thayer); p. 1262 (Cong. Broomall); p. 1266 (Cong. Raymond); p. 1295 (Cong. Latham); and pp. 1832, 1836 (Cong. Lawrence). This understanding of a congressional civil rights enforcement authority was expressed outside of Congress as well. See the *New York Tribune* (February 3, 1866): 6; *Chicago Tribune* (February 4, 1866): 2; *New York Times* (January 16, 1866): 4; (February 8, 1866): 4; (March 31, 1866): 4; *Baltimore American* (March 23, 1866), clipping in Scrapbook on the Civil Rights Bill, Edward McPherson, ed., Edward McPherson Papers, Library of Congress, container 99, p. 4; and various undated clippings in the Scrapbook from the *New York Sun*, p. 15; *Pittsburgh Gazette*, p. 40; *Cincinnati Gazette*, pp. 40, 54; *Syracuse Journal*, p. 53; *Cleveland Herald*, p. 54; and the *Chicago Post*, p. 58, among others.

[11] *Congressional Globe*, 39th Cong., 1st sess., 1866, pp. 1118–19. I established that this was also the understanding of U.S. judges and attorneys in *Politics of Judicial Interpretation*, 1–12, 27–48.

[12] Section 1 provides that such citizens "shall have the same right, in every State and Territory in the United States, to make and enforce contracts, to sue, be parties, and give evidence, to inherit, purchase, lease, sell, hold, and convey real and personal property, and to full and equal benefit of all laws and proceedings for the security of person and property, as is enjoyed by white citizens, and shall be subject to like punishment, pains, and penalties, and to none other, any law, statute, ordinance, regulation, or custom, to the contrary notwithstanding"; 14 Stat. 27 (1866).

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interpreting the Thirteenth Amendment as a mere abrogation of slavery.[13] The citizenship clause of the Fourteenth Amendment makes explicit the constitutional recognition of the status and natural rights of citizens that its framers believed was implied in the Thirteenth Amendment. The ratification of the Fourteenth Amendment in 1868 thus completed the constitutional revolution regarding citizenship and civil rights. Congressional Republicans legislated to secure the civil rights of Americans in 1866 with the understanding that, with the Thirteenth and then the Fourteenth Amendment, the Constitution of the United States gave to all Americans the fundamental rights of citizenship and delegated to Congress the authority to protect citizens in their enjoyment of these rights.[14]

A striking feature of the framers' intent in 1866 is their adoption of the most radical abolitionist theory of constitutionalism before the Civil War.[15] By 1866, not only radicals but all moderate and even some conservative Republicans supported the efforts of Congress to secure civil rights.[16] This shift reveals the extent to which the Civil War radicalized American politics. A political and constitutional position regarded as extreme and embraced by a very small minority before the Civil War had become mainstream Republicanism by 1866. The position that contemporaries regarded as radical in 1866 was securing the voting rights of blacks. As a matter of law and as a matter of political objectives, most contemporaries distinguished between civil rights and voting rights.[17] The essential reason that Radical Republicans criticized the Fourteenth Amendment as too moderate was its failure to provide the same protection for voting rights as for civil rights.[18]

The full reach of this revolution in constitutionalism could have changed the nature of American government from a federal republic with divided authority to a unitary state. Democratic opponents in Congress recognized the implications of the Republicans' theory of constitutionalism. Democrats objected to the proposed Fourteenth Amendment and Civil Rights Bill precisely because the

[13] *Congressional Globe*, 39th Cong., 1st sess., 1866, pp. 1291–92 (Cong. Bingham); and p. 2896 (Sen. Howard).

[14] Supporters of civil rights enforcement insisted that U.S. citizenship, and not state citizenship, entitled Americans to all of the fundamental rights of citizenship and the enjoyment of these rights. See the debates among Senators Van Winkle, Cowan, Trumbull, Johnson, Davis, and Clark in *Congressional Globe*, 39th Cong., 1st sess., 1866, pp. 497–500, 506, 523–30, 600, 1777–81, and Appendix 182. Opponents of congressional civil rights enforcement denied that Congress possessed the authority to determine the status and guarantee the rights of citizens within the states. *Congressional Globe*, 39th Cong., 1st sess., 1866, p. 1156 (Cong. Thornton); p. 1268 (Cong. Kerr); p. 1679 (Pres. Johnson's veto of the Civil Rights Bill); and pp. 1775–80 (Sen. Johnson). For a fuller discussion of these points, see Kaczorowski, "Revolutionary Constitutionalism."

[15] For a good analysis of antebellum abolitionist theories of constitutionalism and slavery, see William Wiecek, *The Sources of Antislavery Constitutionalism in America, 1760–1848* (Ithaca, N.Y., 1977). See also tenBroek, *Equal under Law*; and Graham, "Antislavery Backgrounds of the Fourteenth Amendment."

[16] See Benedict, *A Compromise of Principle*; Cox and Cox, *Politics, Principle, and Prejudice*.

[17] Good discussions of the political aspects of the suffrage issue are in La Wanda Cox and John Cox, "Negro Suffrage and Republican Politics: The Problem of Motivation in Reconstruction Historiography," *Journal of Social History*, 33 (1967): 303; William Gillette, *The Right to Vote: Politics and the Passage of the Fifteenth Amendment* (Baltimore, Md., 1965). For representative comments in Congress on the issue of black suffrage, see *Congressional Globe*, 39th Cong., 1st sess., 1866, pp. 476, 599 (Sen. Trumbull). For representative press commentary, see *The Nation*, 2 (April 26, 1866): 518; and the *Chicago Tribune* (February 5, 1866): 4.

[18] See *Congressional Globe*, 39th Cong., 1st sess., 1866, pp. 673, 675, 684 (Sen. Sumner); and Kaczorowski, "Nationalization of Civil Rights."

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constitutional theory that these measures encompassed could be used by Congress to destroy the civil and criminal authority of states over their citizens. If, as proponents of civil rights insisted, the Constitution guaranteed the fundamental rights of citizenship, Congress could exercise exclusive jurisdiction over civil rights. National law could supplant state law and the national government could absorb "all reserved state sovereignty and rights."[19] Senator Garret Davis, Democrat from Kentucky, was one of several opponents who objected that "the principles involved in this bill, if they are legitimate and constitutional, would authorize Congress to pass civil and criminal codes for every State of the Union."[20] These views were echoed by the House of Representatives, the White House, and the press.[21]

These positions were a continuation of a constitutional battle that had raged for many years. Before the Civil War, the states had defined the status and secured the rights of the inhabitants of the United States. They performed these functions through state legal institutions, statutes, and courts. Some antebellum legal theorists argued, however, that the primary authority to perform these functions rested with the national government.[22] The question of whether the national or state governments possessed ultimate authority to determine the status and enforce the rights of American inhabitants produced a national political and constitutional debate that centered on slavery and culminated in the South's secession in 1861. Secession, based on the constitutional theory of state sovereignty, made the legal questions of federalism and the locus of sovereignty central issues of the Civil War. The North responded with Abraham Lincoln's theory of national sovereignty, which denied the existence of any state's right to secede.[23] The Emancipation Proclamation added the other central question, namely, which government possessed the primary constitutional authority to determine the status of American inhabitants.

The antebellum constitutional questions of the nature of American federalism, the locus of sovereignty, and the primary authority over the status of Americans were thus joined as political issues in the Civil War. The causes of Unionism, national sovereignty, and emancipation were victorious on the battlefield. North-

[19] *Congressional Globe*, 39th Cong., 1st sess., 1866, Appendix 185, pp. 156–58 (Sen. Davis).

[20] *Congressional Globe*, 39th Cong., 1st sess., 1866, p. 1414.

[21] *Congressional Globe*, 39th Cong., 1st sess., 1866, pp. 1120, 1122, 2538 (Cong. Rogers); p. 1154 (Cong. Eldridge); p. 1156 (Cong. Thornton); p. 1266 (Cong. Raymond); pp. 1268, 1270–71 (Cong. Kerr); p. 1295 (Cong. Latham); pp. 1679–81 (Pres. Johnson's veto); ex-Governor William Sharkey to Mississippi Governor Benjamin Humphreys, 17 September 1866, reprinted in unidentified newspaper, clipping in Scrapbook on the Fourteenth Amendment, Edward McPherson Papers, Library of Congress, container 100, p. 23; resolution of the Texas legislature rejecting ratification of the Fourteenth Amendment, reprinted in the *New York Times* (November 4, 1866): 2; resolution of the Kansas Senate Committee on the Fourteenth Amendment, reprinted in the *Memphis Avalanche*, n.d., clipping in Scrapbook on the Fourteenth Amendment, Edward McPherson Papers, p. 55.

[22] John Hurd, *The Law of Freedom and Bondage in the United States*, 2 vols. (Boston, 1858, 1862); James Kettner, *The Development of American Citizenship, 1608–1870* (Chapel Hill, N.C., 1978); and Wiecek, *Sources of Anti-Slavery Constitutionalism*.

[23] These conflicting constitutional theories were expressed in the Southern state resolutions relating to secession and in President Lincoln's messages to Congress. "Mississippi Resolution on Secession," in *Documents in American History*, Henry Commager, ed. (Englewood Cliffs, N.J., 1973), 373; "South Carolina Declaration of Causes of Secession," p. 372; "President J. Davis, Message to Congress," p. 389; Abraham Lincoln, "Message to Congress in Special Session, July 4, 1861," p. 393.

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ern Republicans believed that the Civil War had resolved these political and constitutional questions. They soon discovered they were mistaken.[24] Former Confederates tenaciously adhered to a philosophy of state sovereignty and refused to respect national authority. They defiantly resisted the emancipation guaranteed blacks by the Thirteenth Amendment. Southern white supremacists denied the freedmen's freedom by continuing to treat them as if they were slaves. White supremacists frequently met the attempts of freed blacks to assert their constitutionally guaranteed freedom with violent repression and economic intimidation. Moreover, they treated white Unionists and federal officers with disrespect, and resorted to economic intimidation and violence toward them as well.[25]

Local officials in the South sanctioned and legitimized the defiant behavior of individuals and the racial and political customs of communities dominated by whites. In their constitutions and laws, Southern states refused to recognize that blacks were citizens possessing the natural rights of free people.[26] State officers commonly failed or refused to protect the personal safety and property of blacks. They similarly refused to extend this protection to whites who were political allies or federal agents of blacks. When Southern blacks and politically unpopular whites were the victims of crimes, they could not get sheriffs to arrest, courts to try, or juries to convict the perpetrators. When charged with crimes or sued in the civil courts, blacks seldom received impartial justice. Indeed, white Unionists and freed blacks were prosecuted and sent to prison during peacetime for aiding the U.S. forces during the war. Southern hostility persuaded Northern Republicans and Southern Unionists that secessionist and Confederate sentiments had survived the Civil War.[27] By the end of 1865, the constitutional and political process of restoring the Southern states to the Union had become the problem of preserving the principles for which the war had been fought.

[24] According to Eric McKitrick, Southern recalcitrance immediately after Appomattox was perceived by Northern Republicans as endangering the fruits of the Union victory. The refusal of many former Confederates to accept defeat necessitated the measures of Reconstruction, in the minds of Northern Republicans. McKitrick, *Andrew Johnson and Reconstruction*, 21–41.

[25] Michael Perman, *Reunion without Compromise: The South and Reconstruction, 1865–1868* (Cambridge, 1973); Leon Litwack, *Been in the Storm So Long: The Aftermath of Slavery* (New York, 1979). Congressional Republicans received reports of these conditions while they were framing the Fourteenth Amendment and the Civil Rights Act of 1866. See Grant Goodrich to Lyman Trumbull, 1 February 1866, Lyman Trumbull Papers, Library of Congress; Brigadier General J. W. Sprague to John Sherman, 4 April 1866, John Sherman Papers; George A. Custer to Zachariah Chandler, 14 January 1866, Zachariah Chandler Papers, Library of Congress. These reports were confirmed by Congress's own investigations. See Joint Committee on Reconstruction, *Report*, 39th Cong., 1st sess., 1866; *Freedmen's Affairs*, 39th Cong., 1st sess., Senate Exec. Doc. 66 (1866); and *Murder of Union Soldiers*, 39th Cong., 2d sess., H. Rept. 23 (1866).

[26] Kaczorowski, *Politics of Judicial Interpretation*, 27–44; Niemen, *To Set the Law in Motion*; James Sefton, *The United States Army and Reconstruction, 1865–1877* (Baton Rouge, La., 1967); Theodore Wilson, *The Black Codes of the South* (Tuscaloosa, Ala., 1965).

[27] The conclusion that Southerners abused white Unionists and freed blacks and that observers interpreted this abuse as hostility toward and resistance to federal authority was expressed in letters to congressional Republicans, such as those cited in note 24 above and the following: William Ware Peck to Charles Sumner, 1 January 1866, Charles Sumner Papers, Houghton Library, Harvard University; and George W. Kingsbury to Justin S. Morrill, 18 June 1866, Justin S. Morrill Papers, Library of Congress.

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THE CIVIL WAR HAD BEEN A UNIQUELY PARTISAN WAR. Republicans stood for Union and emancipation; Democrats were associated with leniency toward Confederate secessionists and slavery. When President Andrew Johnson attempted a quick restoration of the Confederate states after the war, Democrats rallied behind him. Restoration by the president presented insuperable political problems, however. It preserved the political leadership of the same individuals and groups that had led the Southern states into secession and civil war.[28] If Republicans acquiesced in Johnson's plan, their Democratic opponents would gain ascendancy by joining with the late belligerents of the South, an outcome unacceptable to Northern Republicans. It would condone "traitorism" and betray their Unionist allies in the South. But achievement of Republican political objectives in the South, which included a political power base of white Unionists and free blacks, would require a change in Southern political leadership. Political participation of the Republicans' Southern allies necessitated a federal presence to protect them from the hostility of the dominant Democratic Conservative white supremacists. As the Civil War was a partisan effort associated with and led by the Republican party, so Reconstruction was a partisan postwar readjustment controlled by and identified with Northern Republicans.

In 1866, the political context of civil rights deprivations compelled Congress to take effective measures to secure the fundamental rights of American citizens. Although Republicans shrank from providing freed blacks with economic independence through land redistribution, they did offer legal recognition of their liberty by securing important rights for their economic autonomy, such as the rights to enter into contracts and to buy and sell property. Congressional Republicans put aside racial prejudice that ordinarily would have precluded the legal enforcement of civil rights.[29] The factors motivating them included the perceived need to preserve the objectives for which so many thousands gave their lives, the obligation to make effective the freedom they had promised to Southern blacks, a sense of elemental fairness and justice, as well as political self-interest. All these objectives were served by providing for the personal safety and security of Southern political allies—civilians of both races and federal officers. The political ideology of the Republican party further diminished the effects of Northern white racism on congressional Republicans. The central ideas of the party were the theory of natural rights, a classic liberalism, and a belief in equal opportunity. The combination served as a concept of American nationalism, distinguishing Repub-

[28] G. Koerner to Lyman Trumbull, 11 January 1866, Trumbull Papers, Library of Congress; H. B. Allis to Benjamin F. Wade, 21 March 1866, Benjamin F. Wade Papers, Library of Congress; Joel Silbey, *A Respectable Minority: The Democratic Party in the Civil War Era, 1860–1868* (New York, 1977); Perman, *Reunion without Compromise.*

[29] Useful discussions of white racism and its effect on the politics of the era include George Fredrickson, *The Black Image in the White Mind: The Debate on Afro-American Character and Destiny, 1817–1914* (New York, 1963); Forrest Wood, *Black Scare: The Racist Response to Emancipation and Reconstruction* (Berkeley, Calif., 1970); V. Jacque Voegeli, *Free But Not Equal: The Midwest and the Negro during the Civil War* (Chicago, 1967); C. Vann Woodward, "Seeds of Failure in Radical Race Policy," *American Philosophical Society Proceedings*, 110 (1966): 1.

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lican notions of Unionism and American freedom from the Southern Democratic Conservative ideology of states' rights and slavery.[30]

Northern Republicans decided that the preservation of American nationhood and freedom, as they understood them, required a strong central government to combat the danger posed by Southern recalcitrance. Republican William Lawrence of Ohio invoked political necessity when he warned the House that the congressional protection of civil rights was "essential to preserve national life, and the means of national existence."[31] Withholding this protection would be tantamount to permitting the Southern states to divest citizens of their rights in the aftermath of Appomattox. The editor of the *Philadelphia American* echoed this theme in urging ratification of the Fourteenth Amendment: "If there be one lesson written in bloody letters by the [Civil] War, it is that the national citizenship must be paramount to that of the State. We propose to make it so . . . This citizenship provision is one of the most vital principles developed by the war. Without it we shall inevitably be exposed to new wars of Secession and States rights and nullification."[32] The governor of Wisconsin, Lucius Fairchild, transmitted a copy of the proposed Fourteenth Amendment to the state legislature and urged ratification "because, in view of the terrible events of the past five years, we deem these guarantees necessary to the life of the nation, and we insist that those who saved that life have an undeniable right to demand full guarantees to its future preservation."[33]

The conjunction of political ideology and political necessity resulted in congressional Republicans embracing a revolutionary theory of constitutionalism. To achieve political power in the South, to preserve their wartime objectives of Unionism and freedom for slaves, they insisted that sovereignty resided in the federal government and included primary authority to determine the status and secure the rights of all Americans, white and black. Republican supporters of the Reconstruction amendments and the civil rights statute acknowledged the revolutionary changes they had wrought in American federalism by delegating plenary authority over citizenship and civil rights to the national government.[34]

[30] For analyses of the Republican party's ideology, see Eric Foner, *Free Soil, Free Labor, Free Men: The Ideology of the Republican Party before the Civil War* (London, 1970); Yehoshua Arieli, *Individualism and Nationalism in American Ideology* (Cambridge, 1964). Examples of this Republican ideology include Sen. Lyman Trumbull's speech in the *Congressional Globe*, 39th Cong., 1st sess., 1866, p. 474; John Andrews, *Valedictory Address of his Excellency John A. Andrews to the Two Branches of the Legislature of Massachusetts, Jan. 4, 1866* (Boston, 1866); Benjamin Butler, *The Status of the Insurgent States upon Cessation of Hostilities: Speech delivered before the Pennsylvania Legislature, Apr. 11, 1866* (Washington, D.C., 1866).

[31] *Congressional Globe*, 39th Cong., 1st sess., 1866, pp. 1832, 1836. See also p. 1090 (Cong. Bingham); pp. 1262–63 (Cong. Broomall); and Appendix 99 (Sen. Yates).

[32] Clipping in Scrapbook on the Fourteenth Amendment, Edward McPherson Papers, Library of Congress, p. 41.

[33] Unidentified newspaper in Scrapbook on the Fourteenth Amendment, p. 63.

[34] See *Congressional Globe*, 39th Cong., 1st sess., 1866, pp. 1065–67 (Cong. Higby); p. 1066 (Cong. Price); p. 2442 (Sen. Howard); pp. 2534–35 (Cong. Ecklay); p. 2961 (Sen. Poland). See above, notes 9, 10, 11, and 31 for evidence that the framers' view of constitutionalism, as it was expressed in Congress and discussed in the press, held that Congress possessed plenary authority to secure civil rights. Opponents objected because they understood that Republican constitutionalism was revolutionary. See pp. 1063–64 (Cong. Hale); pp. 1083–85 (Cong. Davis); p. 2500 (Cong. Shanklin); p. 2538 (Cong. Rogers); p. 2987 (Sen. Cowan); and p. 3147 (Cong. Harding).

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Before the Civil War, the states had exercised almost exclusive jurisdiction over fundamental rights. Under the Thirteenth and Fourteenth Amendments, as Republicans understood them, Congress could conceivably supplant the states in securing civil rights. By virtue of the Constitution's supremacy clause, Congress could exercise exclusive authority over citizenship and civil rights and thereby destroy state authority as a matter of constitutional law.[35] Indeed, Congress exercised this authority when it determined by statute and constitutional amendment which people were citizens and what rights they were to enjoy. The states were deprived of their historical authority to make these decisions. Although congressional Republicans acknowledged the constitutional revolution in which they were engaged, they carefully avoided carrying this revolution to its ultimate conclusion of creating a unitary political structure. Republicans did not wish to supplant the states in providing a foundation for ordinary civil and criminal justice. On the contrary, they consciously preserved federalism by avoiding unnecessary intrusions on state authority over civil rights. Intentionally recognizing concurrent authority, Congress restricted its protection of fundamental rights to situations in which states and localities failed to protect them.

The decision of congressional Republicans to preserve state authority over ordinary civil and criminal justice has led legal historians to conclude that the Thirteenth and Fourteenth Amendments and the Civil Rights Act of 1866 were modest increases in national authority.[36] The evidence, however, supports the belief of the framers that the Thirteenth and Fourteenth Amendments and the 1866 statute would bring about revolutionary changes in federal constitutionalism. The underlying constitutional authority to enforce civil rights stemmed from the constitutional recognition of the status and rights of free people as having the status and rights of U.S. citizens. Because the civil rights inherent in citizenship were constitutionally guaranteed, the framers believed that Congress, by statute and constitutional amendment, could require the states, the traditional guardians of these rights, to secure them for all Americans. Congress conferred citizenship on all qualified persons in Section 1 of the Civil Rights Act and the Fourteenth Amendment, the framers thereby forcing the states to recognize freed blacks as citizens both of the United States and of the states of their residence. The states no longer could decide state citizenship for themselves. They were prohibited from arbitrarily excluding any qualified persons from state citizenship and from refusing to secure the civil rights to which citizenship entitled them. By defining natural rights as constitutionally recognized rights of American citizenship, Republicans acknowledged that Americans possessed these rights independent of state law. That is, if the states were to repeal their legal recognition of these rights, citizens could still claim them as constitutionally recognized and secured rights of

[35] This exclusive authority was precisely one of the consequences of Republican constitutionalism to which opponents adamantly objected. A modern example of the use of the supremacy clause is the exercise by Congress of exclusive authority over interstate commerce, known as the preemption doctrine. See *Perez v. Campbell*, 402 U.S. 637 (1971); *Pennsylvania v. Nelson*, 350 U.S. 497 (1956); *Rice v. Santa Fe Elevator Corp.*, 331 U.S. 218 (1947).

[36] These historians are cited in note 7.

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American citizenship. Indeed, James F. Wilson of Iowa made this precise point in arguing that the bill was needed because the Southern states failed to recognize and secure the rights of certain Americans. Congress had to provide this protection and had the power to do so independent of the states.[37]

CERTAIN ASPECTS OF THE FRAMERS' UNDERSTANDING of civil rights enforcement were evidently vague and ambiguous. Determining from the language of the law the specific civil rights that individuals possessed as citizens was problematical. President Lincoln's attorney general, Edward Bates, in an official opinion, discussed the ambiguous definition of the rights of citizens as "generic, common, embracing whatever may be lawfully claimed."[38] The framers acknowledged the legal uncertainty, yet they were unequivocal in declaring that the natural rights to life, liberty, and property, and rights incidental to these, were the rights of U.S. citizenship that they intended to secure with the Civil Rights Act and the Fourteenth Amendment.[39] The representative from Ohio, William Lawrence, explained the Republican legal theory of civil rights enforcement and the generic nature of the rights of citizens that Republicans were attempting to guarantee: "It is idle to say that a citizen shall have the right to life, yet to deny him the right to labor, whereby alone he can live. It is a mockery to say that a citizen may have a right to live, and yet deny him the right to make a contract to secure the privilege and the rewards of labor . . . Every citizen, therefore, has the absolute right to live, the right of personal security, personal liberty, and the right to acquire and enjoy property. These are rights of citizenship. As necessary incidents of these absolute rights, there are others, such as the right to make and enforce contracts, to purchase, hold, and enjoy property, and to share the benefit of laws for the security of person and property."[40]

This Republican theory of citizenship and congressionally enforceable civil rights led to two important legal consequences. In defining civil rights and the privileges and immunities of U.S. citizens as the natural rights of free people, the framers provided legal recognition of the civil rights of citizens independent of state law. In addition, the framers established an expansive body of nationally enforceable civil rights. Wilson explained that "there is no right enumerated in the Constitution by general terms or by specific designation which is not definitely embodied in one of the rights I have mentioned, or results as an incident necessary to complete defense and enjoyment of the specific right."[41] The framers created the potential for a future Congress or the courts to enforce rights that were not specified in the Civil Rights Act, or that, in 1866, may not have been regarded as incident to the natural rights to life, liberty, and property encompassed within the privileges and immunities clause of the Fourteenth Amendment.

[37] *Congressional Globe*, 39th Cong., 1st sess., 1866, pp. 1117–18.
[38] E. Bates, "Citizenship," *Official Opinions of the Attorneys General*, 10 (1866): 383, 407.
[39] Cong. Shellabarger complained, "It has been found impossible to settle or define what are all the indispensable rights of American citizenship." *Congressional Globe*, 39th Cong., 1st sess., 1866, p. 1293.
[40] *Congressional Globe*, 39th Cong., 1st sess., 1866, p. 1756.
[41] *Congressional Globe*, 39th Cong., 1st sess., 1866, pp. 1118–19.

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Although the enactments of 1866 were radical, the framers' concept of a national authority to enforce civil rights was developmental and expansionary. It permitted civil rights protection beyond that provided in 1866. Congress subsequently attempted to enforce additional rights more directly. In 1870 and 1871, for instance, Congress passed laws that protected political participation and political expression by criminalizing certain conspiracies intended to prevent people from enforcing their rights of U.S. citizenship. In 1875, Congress legislated to desegregate places of public accommodations.[42] Judges reflected this developmental aspect of Republican constitutionalism in interpreting the Civil Rights Act of 1866 and the Fourteenth Amendment as securing rights not enumerated in the statute's first section. Federal judges applied the Civil Rights Act of 1866 to segregated public facilities and common carriers and held the proprietors liable under the first and second sections of the statute for infringing the rights of citizens to equal access. Courts sometimes ordered court officers to allow blacks on juries, even though the Act did not explicitly guarantee the right to serve or the defendant's right to racially integrated juries. Federal judges interpreted the privileges and immunities of citizenship secured by the Fourteenth Amendment as including the guarantees of the Bill of Rights, such as freedom of speech and freedom of assembly.[43]

The framers drafted the Civil Rights Act of 1866 to describe some of the civil rights to which U.S. citizenship entitled citizens, that is, civil rights that the framers believed incidental to the generic rights of life, liberty, and property. Section 1 declares that U.S. citizens "of every race and color . . . shall have the same right . . . as is enjoyed by white citizens."[44] This language appears to confer on citizens only a right to racial equality and not the substantive rights *per se*. In wording Section 1 as they did, however, the framers sought to secure these rights of citizenship in two ways: by requiring the states to impartially protect the personal rights and safety of all persons through systems of civil and criminal justice, and by providing a cause of action for those unable to enjoy rights as white citizens enjoyed them.

The framers permitted the states to decide for themselves the conditions under which civil rights were to be exercised and enjoyed, as long as the conditions were reasonable. Congress permitted the states to vary the enjoyment of civil rights according to age, sex, mental capacity, and alienage, since such discriminations were considered reasonable and necessary.[45] Although Republicans envisioned a

[42] 16 Stat. 140 (1870); 17 Stat. 13 (1871); 18 Stat. 335 (1875).

[43] See Scrapbook on the Civil Rights Bill, Edward McPherson Papers, Library of Congress, pp. 108–11, 119–20, 133–34, 136; and Scrapbook, John Underwood, ed., John C. Underwood Papers, Library of Congress, pp. 193, 203, 205, 207. See the *New York Times* (May 8, 1867): 1; (August 9, 1867): 4; (August 30, 1867): 5; (October 17, 1867): 1; (October 20, 1867): 1. See also *Slaughterhouse Cases*, 15 F. Cas. 649, 652–53 (C.C. La. 1870) (no. 8408); *United States v. Hall*, 26 F. Cas. 79, 81–82 (C.C. La. 1871) (no. 15,282); *United States v. Given*, 25 F. Cas. 1328, 1329 (C.C. Del. 1873) (no. 15,211). For a contrary view, see Nieman, *To Set the Law in Motion*, 139.

[44] Section 1 specifies "that all persons born in the United States and not subject to any foreign power, excluding Indians not taxed, are hereby declared to be citizens of the United States"; 14 Stat. 27 (1866).

[45] The *New York Evening Post* commented that the Civil Rights Act declared who were citizens and what rights they had. Yet the *Post* also insisted that the statute did not "usurp the power of the local

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national uniformity in the rights that citizens possessed as citizens, they also provided for local variations in the ways in which citizens exercised these rights. The framers thus preserved a federal constitutional structure of government that distributed authority over fundamental rights to both the national and state governments but delegated ultimate authority to the national government.

Judges understood the Republican legal theory of civil rights enforcement authority that underlay the Civil Rights Act of 1866 to be a blend of concurrent authority and exclusive national authority. Although the fundamental rights of citizenship, such as those specified in the Civil Rights Act, were secured to all citizens under national law, and the states could not deny these rights to any citizen, the states were still permitted to determine the conditions under which these civil rights were to be exercised, so long as state law did not conflict with national law or until Congress exercised the authority to prescribe these conditions. In 1867, Justice Noah H. Swayne clarified the scope of this theory, deciding that the Kentucky rules of evidence violated the Civil Rights Act of 1866. These rules recognized the right of blacks to testify in Kentucky state courts, but only if no white witnesses were available. Observing that the Thirteenth Amendment "reversed and annulled the original policy of the constitution" by guaranteeing to all Americans the status and rights of free people and delegating to Congress the authority to secure the status and rights of citizenship, Justice Swayne held that the Civil Rights Act guaranteed black witnesses the right to testify under the same conditions white witnesses did.[46] On the other hand, the California Supreme Court stated in 1869 that if "title to real property of any character may be conveyed by writing not under seal, then all citizens, of every race and color, may convey property of that character in the same mode."[47] California's acceptance of this method did not compel any other state to allow similar conveyances. The California rule was tenable under national law because it was a reasonable regulation of a congressionally enforceable right of U.S. citizenship. The Civil Rights Act of 1866 and the Fourteenth Amendment thus maintained a concurrence in national and state authority over civil rights.

Even though fundamental rights of citizens were nationalized, property law, contract law, and tort law, among others, remained under state jurisdiction, as did criminal law. The deliberate perpetuation of the traditional role of the states in securing civil rights, under national scrutiny, is understandable in light of the realities that confronted Congress in 1866. Despite the vast increase in the power

legislature to prescribe in what manner the rights of person and property shall be secured. A distinction is to be taken here which the President's Veto message overlooks. Congress does not in this bill say by what rules evidence shall be given in courts, by what tenure property shall be held, or how a citizen shall be protected in his occupation. It only says to the states, whatever laws you pass in regard to these matters, make them general; make them for the benefit of one race as well as for another." Clipping in Scrapbook on the Civil Rights Bill, Edward McPherson Papers, Library of Congress, p. 32.

[46] *United States v. Rhodes*, 27 F. Cas. 785, 794 (C.C. Ky. 1867) (no. 16,151). By 1869, the states generally recognized the right of blacks to testify as a congressionally secured right of citizenship. *Ex parte Warren*, 31 Tex. 147 (1868); *Kelley v. Arkansas*, 25 Ark. 392 (1869); *State v. Washington*, 36 Cal. 658 (1869); *State v. Underwood*, 63 N.C. 98 (1869).

[47] *State v. Washington*, 36 Cal. 666–67 (1869).

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of the national government and the heightened sense of nationalism generated by the Civil War, the states were still better suited to be the guardians of civil rights. The national government simply did not have the resources to take on this new function without the assistance of state governments. Nor was it necessary, let alone desirable, for the national government to supplant state civil and criminal codes, courts, and enforcement agencies, or to handle ordinary crimes and civil suits.[48]

Another point of debate over the framers' concept of a congressional civil rights enforcement authority concerns whether the framers merely intended to secure a racial equality in state law or whether they intended to protect rights directly. I have argued that the framers believed Congress possessed the authority to enforce civil rights directly and to redress any violations of civil rights. Whether or not Congress chose to exercise this authority to its full extent, however, is another question. The framers' understanding of the full scope of congressional authority to enforce civil rights is a separate question from that of the scope of authority the framers exercised to enforce civil rights in 1866. Whatever the framers' intent, they permitted the statute and the amendment to go forward with the revolutionary potential that a broad reading entailed.

Federal legal officers and judges certainly interpreted and applied the statute to secure more than racially impartial state law. The Civil Rights Act authorized these law enforcement officers to directly redress civil and criminal violations of civil rights. Sections 1 and 3, as they were applied by federal officers, created a private civil cause of action for the infringement of the rights that the Civil Rights Act secured.[49] Section 2 created a misdemeanor punishable in the federal courts whenever a person, acting under pretext of law, statute, ordinance, regulation, or custom, violated the civil rights secured by Section 1.[50] Section 3 authorized the federal courts to supplant state and local courts and to sit as courts of primary civil and criminal jurisdiction in specified circumstances. It provided for the removal to the federal district and circuit courts of "all causes, civil and criminal, affecting persons who are denied or cannot enforce in the courts or judicial tribunals of the state or locality where they may be any of the rights secured to them by the first section." If local law enforcement officers failed to arrest and hold for trial alleged criminals, the military and federal marshals were authorized to do so; if the state court then failed to try the defendants, they were to be tried in the federal court.[51]

[48] Nevertheless, the creation of the Department of Justice and the reorganization of the office of Attorney General in 1870 was a congressional recognition of the expanded jurisdiction and increased legal business of the national government. This reorganization was an attempt by Congress to increase efficiency in the handling of the nation's burgeoning legal affairs. However, Harold Hyman has noted an anti-bureaucratic bias in this period that sharply curtailed the effectiveness of governmental action in meeting the needs of Americans. Kaczorowski, *Politics of Judicial Interpretation*, 79; Hyman, *A More Perfect Union*, chaps. 17–22, 25.

[49] *In re Turner*, 24 F. Cas. 337 (C.C. Md. 1867) (no. 14,247); Kaczorowski, *Politics of Judicial Interpretation*, 27–48.

[50] Section 1 of the Civil Rights Act of 1866 refers to citizens and specifies some of the rights that citizens are to enjoy; Section 2 specifies certain infringements of these rights regardless of whether or not the victim is a citizen. 14 Stat. 27 (1866).

[51] Federal legal officers—U.S. attorneys, marshals, and Freedmen's Bureau agents—and federal judges interpreted the Civil Rights Act in this way. They were instrumental in prosecuting crimes committed by whites against blacks when, immediately after the Civil War, local law enforcement

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Section 3 also authorized removal in situations in which a state defendant, civil or criminal, could not receive impartial justice. In each of these instances, federal agencies and officers exercised primary jurisdiction and administered justice as if they were state and local agencies and officers.

OTHER ENFORCEMENT PROVISIONS OF THE CIVIL RIGHTS ACT were taken directly from the Fugitive Slave Act of 1850, which had been adopted to implement the fugitive slave clause of the Constitution.[52] The statute authorized, even required, federal judges and law officers to return runaway slaves to their owners. The Supreme Court had interpreted the fugitive slave clause as conferring on slaveowners an unqualified right of property in their slaves, a right that delegated to "the national government . . . appropriate authority and functions to enforce it" through national legislative, judicial, and executive agencies independent of the states.[53] The Fugitive Slave Act of 1850 secured a fundamental right of U.S. citizenship and formed a model for the Civil Rights Act of 1866. The framers recognized the irony of using a statute adopted to assure the rights of slaveowners as the model for legislation to secure the rights of their former slaves. Senator Lyman Trumbull of Illinois approved this connection when he admonished the Congress: "Surely we have the authority to enact a law as efficient in the interests of freedom, now that freedom prevails throughout the country, as we had in the interest of slavery when it prevailed in a portion of the country."[54] The Fugitive Slave Act of 1850 served the cause of civil rights in 1866 in two ways. It provided a blueprint for the direct enforcement of constitutional rights in national courts by national legal agencies. It also provided a precedent for congressional legislation enacted to enforce rights over which the states had previously exercised jurisdiction.

Some supporters of the bill explicitly expressed their intention to delegate to the federal courts the primary jurisdiction over civil rights. When Wilson introduced the Civil Rights Bill in the House, he declared that U.S. citizens were entitled to the great fundamental rights of life, liberty, and property that free governments are obliged to secure, and that, because the states refused to protect these rights for some citizens, "we must do our duty by supplying the protection which the states deny." The Republican senator from Indiana, Henry S. Lane, expressed the same understanding in the Senate and declared: "Neither the judge, nor the jury, nor the officer as we believe is willing to execute the law . . . We should not legislate at all if we believed the State courts could or would honestly carry out the provisions of the Thirteenth Constitutional Amendment; but because we believe they will not

agencies would not. See Kaczorowski, *Politics of Judicial Interpretation*, 8–12, 27–48. The Supreme Court subsequently interpreted Section 3 narrowly in a technical ruling that eliminated the criminal jurisdiction over these crimes (pp. 135–43). The Supreme Court's decision is cited in *Blyew and Kennard v. United States*, 13 Wall. (80 U.S.) 581 (1872).

[52] 27 Stat. 14, sects. 4–9 (1866); 9 Stat. 462 (1850).

[53] *Prigg v. Pennsylvania*, 16 Pet. (41 U.S.) 539, 615, 623 (1842).

[54] *Congressional Globe*, 39th Cong., 1st sess., 1866, p. 475.

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do that, we give the Federal officers jurisdiction."[55] When the congressional proponents of civil rights enforcement stated that civil rights were to be secured within the states, and when congressional opponents complained that Congress was changing the relationship between citizens and the state governments, both groups were referring to this assumption of state police powers by federal officers and agencies. Federal legal officers and law enforcement agencies performed state police functions and administered civil and criminal justice when local law enforcement agents and agencies would not.[56]

Congress enacted this statute to enforce civil rights because the Republican majority believed that Congress possessed primary authority over the civil rights of American citizens. Congress would neither have conferred the jurisdiction it did on federal agencies and officers nor legislated to secure civil rights two years prior to the ratification of the Fourteenth Amendment, if a majority of members had believed that these powers were constitutionally reserved to the states, as the Supreme Court declared in 1873 and 1876.[57] The decision by Congress to confer on federal courts the jurisdiction to try criminal offenses and civil disputes supports the view that the framers believed that the Thirteenth Amendment delegated to Congress primary authority over civil rights.

The framers' interpretation of the Thirteenth Amendment as a general guarantee of the natural rights of free people led to their inclusion of white Americans within the protective guarantees of the Civil Rights Act. The framers were explicit in expressing their intention to secure the civil rights of all Americans. Senator Trumbull stated that the Civil Rights Bill "applies to white men as well as black men. It declares that all persons in the United States shall be entitled to the same civil rights."[58] A few years later, to combat the Ku Klux Klan, many of these same senators and representatives adopted additional legislation that defined as federal crimes certain civil rights violations and conspiracies to violate civil rights without requiring racial prejudice as an element of the offense. Also, U.S. attorneys did not charge defendants with racial discrimination to bring violations of civil rights within federal jurisdiction under these statutes. The gist of the offense was the intention to deprive U.S. citizens of their rights.[59]

[55] *Congressional Globe*, 39th Cong., 1st sess., 1866, p. 1118 (Cong. Wilson); pp. 602–03 (Sen. Lane).

[56] See Kaczorowski, *Politics of Judicial Interpretation*, 27–48, 52–53, 79–115.

[57] *Slaughterhouse Cases*, 16 Wall. (83 U.S.) 36 (1873); *United States v. Cruikshank*, 2 Otto (92 U.S.) 542 (1876).

[58] *Congressional Globe*, 39th Cong., 1st sess., 1866, p. 599. See also p. 474 (Sen. Trumbull); pp. 504–05 (Sen. Johnson); p. 595 (Sen. Davis); p. 603 (Sen. Cowan); p. 3035 (Sen. Henderson); pp. 1066–67 (Cong. Price); p. 1117 (Cong. Wilson); pp. 1120–21 (Cong. Rogers); pp. 1263, 1265 (Cong. Broomall); p. 1264 (Spencer Colfax); pp. 1291, 2542 (Cong. Bingham); p. 1853 (Cong. Lawrence). See newspaper clippings on the topic in Scrapbook on the Civil Rights Bill, Edward McPherson Papers, Library of Congress, pp. 4, 26, 32, 37, 47, 53, 58, 62, 78, 79, 132; clippings in Scrapbook on the Fourteenth Amendment, Edward McPherson Papers, pp. 17, 29, 31, 41, 53, 63, 82.

[59] 16 Stat. 140 (1870); 17 Stat. 13 (1871). U.S. District Judge Richard Busteed to Attorney General Amos T. Akerman, 22 November 1871, Source Chronological File, Northern District of Alabama, Department of Justice Records, National Archives; Charge to Grand Jury, enclosed in above letter; U.S. Attorney E. P. Jacobson to Amos Akerman, 4 and 7 August 1871, Source Chronological File, Southern District of Mississippi, Department of Justice, National Archives; Jacobson to Attorney General George Williams, 17 February 1872, *ibid.*; U.S. Attorney G. Wiley Wells to George Williams, 5 March and 2 April 1872, Source Chronological File, Northern District of Mississippi, Department of Justice,

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The recognition of Congress as having the primary constitutional authority over the personal security and civil rights of American citizens presented the danger that the national government would supplant state and local governments in their ordinary police functions. In 1866, opponents of the Civil Rights Bill loudly protested that this was precisely the bill's effect.[60] The framers confronted this difficult problem of preserving federalism when they attempted to make civil rights violations federal crimes punishable in the federal courts. The framers attempted to distinguish federal crimes from ordinary crimes by limiting federal criminal violations of civil rights to acts committed "under color of any law, statute, ordinance, regulation, or custom." Wilson explained that Congress was "not making a general criminal code for the states."[61] On the contrary, the supporters wished to preserve the state administration of ordinary criminal justice. Although the framers of the Civil Rights Acts of 1870 and 1871 expressed the same desire, under these statutes, officers of the Department of Justice and federal judges in some areas of the Southern states did replace local authorities who were overwhelmed by the Klan and administered criminal justice for them.[62]

The framers of the Civil Rights Act of 1866 also limited its scope because they intended to match federal sanctions to the specific violations of civil rights that required national intervention, just as the framers of the 1870 and 1871 statutes specified other kinds of offenses that confronted them. In other words, they shaped federal criminal sanctions to provide effective civil rights protection where and when it was needed. Black victims of racially motivated civil rights violations who were unable to redress their grievances within the state criminal justice system had the greatest need for federal protection in 1866; consequently, the Act made civil rights violations that were motivated by discriminatory intent federal crimes. Senator Trumbull explained that the words, "'under color of law' were inserted as words of limitation . . . If an offense is committed against a colored person simply because he is colored, in a state where the law affords the same protection as if he were white, this act neither has nor was intended to have anything to do with his case, because he has adequate protection in the State Courts; but if he is discriminated against under color of State law because he is colored, *then it becomes necessary to interfere for his protection*."[63] Discriminatory intent conceivably included political prejudice, since white Unionists were also to be protected. Because state

National Archives; U.S. District Judge Robert A. Hill to Solicitor General Benjamin Bristow, 13 May 1872, Benjamin H. Bristow Papers, Library of Congress; Joint Select Committee on the Condition of Affairs in the late Insurrectionary States, *Report*, 42nd Cong., 2d sess., S. Rept. 41 (1871), vol. 12, pp. 934–87. I discuss this subject in *Politics of Judicial Interpretation*, 117–34.

[60] Sen. Willard Saulsbury of Delaware warned that the Civil Rights Bill "positively deprives the State of its police power of government." *Congressional Globe*, 39th Cong., 1st sess., 1866, p. 478. Sen. Davis identified specific areas of state jurisdiction that Democrats feared would be supplanted by the Civil Rights Bill when he espoused the state sovereignty theory of federalism and exclusive state authority over civil rights. See p. 596.

[61] *Congressional Globe*, 39th Cong., 1st sess., 1866, p. 1120.

[62] This history is recounted in Kaczorowski, *Politics of Judicial Interpretation*, 50–134.

[63] *Congressional Globe*, 39th Cong., 1st sess., 1866, p. 1758, emphasis added. Sen. Trumbull's comments were made in rebuttal to Pres. Johnson's objection that the Civil Rights Act would deprive state courts of all cases affecting persons who were discriminated against, not simply those cases involving a discrimination. See Pres. Johnson's veto message on p. 1680.

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laws and courts could be relied on to provide adequate remedies for ordinary violations of the rights to life, liberty, and property, Section 2 of the Civil Rights Act constituted an additional sanction for these violations when they were committed under a discriminatory law or custom. As Senator Trumbull described it, the framers believed a federal sanction was required because victims would be unable to secure their rights when the violations stemmed from discriminatory laws or community prejudices.

The framers of the Civil Rights Act of 1866 and the Fourteenth Amendment did not intend federal jurisdiction over civil rights to be limited to racially discriminatory state action, as the Supreme Court later held in *Slaughterhouse* and *Cruikshank*. Federal agents who removed cases from local authorities under Section 3 assumed their powers were broad. Judge Adjutant General of the Army, Joseph Holt, for instance, interpreted the statute as authorization for removal in a case in which a former Freedman's Bureau agent claimed that he was being harrassed with a false prosecution in a Louisiana court because of the official assistance he had given to blacks. When the state judge, Edmund Abell, refused to allow the case to be removed to the federal district court, federal officers arrested him and charged him with violating the Civil Rights Act.[64] In another case, the U.S. Circuit Court held that white butchers in New Orleans had a claim against a slaughterhouse corporation chartered by the state, which the butchers alleged had interfered with rights they enjoyed under the Civil Rights Act, namely, the rights to labor, enter into contracts, and to equal benefit of the law for protection of person and property.[65]

THE FRAMERS NEVER QUESTIONED THE AUTHORITY OF CONGRESS to confer jurisdiction on the federal courts to try and punish civil rights violators. Congress possessed this authority, they believed, because civil rights were nationally enforceable rights of U.S. citizenship, under the Thirteenth Amendment and Section 1 of the Civil Rights Act.[66] Hence, Congress possessed the authority to prescribe criminal sanctions to secure these rights. The critical issue was whether or not Congress possessed the authority to secure civil rights at all, not whether Congress possessed the authority to provide criminal sanctions for persons who violated its statutes. Congress spent little time discussing whether it could punish private individuals who violated the Civil Rights Bill. If one conceded that Congress possessed the authority to secure civil rights, it was too obvious for discussion that it also possessed authority to punish violators of its statutes that secured civil rights. The issue the framers found troublesome in this regard was the propriety of authorizing the federal courts to punish state officers who violated civil rights when they acted under the authority of state laws. Punishing state

[64] Kaczorowski, *Politics of Judicial Interpretation*, 34, 45, n. 16.

[65] *Slaughterhouse Cases*, 15 F. Cas. 655; reversed on other grounds, *Slaughterhouse Cases*, 16 Wall. (83 U.S.) 36. See also *United States v. Hall*, 26 F. Cas. 79.

[66] The framers generally believed that the authority to secure civil rights of citizens "belongs to every sovereign power, and is essentially a subject of national jurisdiction." *Congressional Globe*, 39th Cong., 1st sess., 1866, p. 1090 (Cong. Bingham).

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officers acting in their official capacities represented a serious breach of federal comity. Confrontations between national and state authority, always sensitive matters, were especially so following the Civil War.

Opponents of the Civil Rights Act questioned the legality of prosecuting state officials for actions that were committed under the sanction of state law.[67] Debate focused on the punishment of state judges who applied racially discriminatory state statutes instead of declaring them unconstitutional. Opponents pointed out the injustice of a public policy that sought to punish a state judge who, believing the Civil Rights Act of 1866 to be unconstitutional, enforced a state statute he believed to be lawful. The criminal prosecution and punishment of state officials was far more controversial than that of ordinary citizens because it involved a direct confrontation between national and state authority. Troubled by this apparent breach in federalism, proponents of the Civil Rights Act nevertheless insisted on the need to punish anyone who violated nationally enforceable civil rights and flouted the statutes Congress enacted to secure them, even if that person were a judge. Senator Trumbull was adamant: "The right to punish individuals who violate the laws of the United States cannot be questioned, and the fact that in doing so they acted under color of law or usage in any locality affords no protection; because by the Constitution that instrument and the laws passed in pursuance thereof are the supreme law of the land, and every judge, not only of the United States, but of every State court is bound thereby."[68]

Even though the inclusion of public officials within the penal sections of the Act was extraordinary, the framers insisted that prosecuting them for civil rights offenses was imperative. Punishing political leaders for civil rights violations would be far more effective in curbing these crimes than punishing ordinary citizens would. Senator Trumbull elaborated: "When it comes to be understood in all parts of the United States that any person who shall deprive another of any right or subject him to any punishment in consequence of his color or race will expose himself to fine and imprisonment, I think such acts will cease."[69] The framers hoped the public would realize that even those thought to be beyond prosecution because they wielded governmental authority were subject to the Act's provisions.[70]

Whereas most of the discussion of Section 2 in Congress focused on state judges as the targets of the penal provisions of the Act, the debates suggest that this section was not intended to apply only to public officials. When Senator Edgar Cowan of Pennsylvania objected that these penal provisions singled out state officials for criminal prosecution, Trumbull denied this. Cowan asked, "Is there not a

[67] *Congressional Globe*, 39th Cong., 1st sess., 1866, pp. 601–02 (Sen. Hendricks); p. 1783 (Sen. Cowan); Appendix 183 (Sen. Davis); pp. 1154–55 (Cong. Eldridge); p. 1265 (Cong. Davis); p. 1267 (Cong. Raymond); and p. 1271 (Cong. Kerr).

[68] *Congressional Globe*, 39th Cong., 1st sess., 1866, p. 1759.

[69] *Congressional Globe*, 39th Cong., 1st sess., 1866, p. 475.

[70] The Department of Justice and the federal courts in some Southern states engaged in a massive effort to bring Ku Klux Klansmen to justice for terrorizing Southern blacks and their white Unionist allies. The government's policy was to select the most prominent defendants for prosecution. Kaczorowski, *Politics of Judicial Interpretation*, 79–99.

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provision by which State officials are to be punished?" Trumbull replied, "Not State officials especially, but everyone who violates the law. It is the intention to punish everybody who violates the law."[71] Trumbull repeated this construction in urging the Senate to enact the Civil Rights Act over President Johnson's veto.[72] Although the language of Section 2 can support a reading that limits its application to state officers, such a reading contradicts Senator Trumbull's explicit interpretation of its scope. In light of the framers' concept of a congressional authority to enforce civil rights that permitted Congress to punish any individual who violated the statute, not to accept the position of Senator Trumbull, the author and Senate floor manager, would be illogical and contradictory to the record, especially when virtually no one in Congress disagreed with him. That federal legal officers and judges interpreted the statute consistent with Senator Trumbull's position makes this conclusion even stronger.[73] It appears, then, that the framers of the Civil Rights Act of 1866 did not intend to apply the criminal penalties of Section 2 only to state officers. Their comments evince their intention to punish even state officials who violated the civil rights of American citizens.

Congressional supporters and opponents of the Civil Rights Act expressed the same concerns and intentions in discussing the statute's third section. This section authorizes removal of a case from the state courts when the parties seeking removal cannot enforce their civil rights or the state's legal process is violating their civil rights. Although the framers expected state agencies to handle ordinary civil and criminal cases, they understood that local officials were failing to protect the rights of many citizens and to administer civil and criminal justice in a racially and politically impartial manner.[74] To make the enforcement of nationally secured rights effective, the framers gave the federal courts the primary civil and criminal jurisdiction over civil rights when state agencies failed to secure them. It is crucial to note that federal legal officers and judges understood that it was the inability of the citizen to enforce a civil right in a state court, not the existence of racially discriminatory state laws, that permitted the individual to remove the case to a federal court.[75] The statute's congressional supporters manifested the same understanding.

The framers thus distinguished ordinary cases in which state agencies enforced and protected the rights of citizens from cases in which the federal courts would have to take jurisdiction because of state intransigence. A party could remove a case to a federal court if the state court enforced discriminatory laws, Senator Trumbull maintained, "or, if undertaking to enforce his right in a State court he was denied that right, then he could go into a Federal court; but it by no means follows that

[71] *Congressional Globe*, 39th Cong., 1st sess., 1866, p. 500.
[72] *Congressional Globe*, 39th Cong., 1st sess., 1866, p. 1758.
[73] Suits were brought against private proprietors of public facilities and public carriers who were fined for excluding blacks from their facilities. See McPherson's and Underwood's Scrapbooks cited in note 43.
[74] *Congressional Globe*, 39th Cong., 1st sess., 1866, pp. 602–03 (Sen. Lane of Indiana); pp. 600, 1760 (Sen. Trumbull); p. 1785 (Sen. Stewart); p. 2967 (Sen. Poland); p. 1119 (Cong. Wilson); p. 1293 (Cong. Bingham); pp. 1293–94 (Cong. Shellabarger); pp. 1832–36 (Cong. Lawrence).
[75] See Kaczorowski, *Politics of Judicial Interpretation*, 8–10.

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every person would have a right *in the first instance to go to the Federal court* because there was on the statute book of the State a law discriminating against him, the presumption being that the judge of the court, when he comes to act on the case, would, in the obedience to the paramount law of the United States, hold the State statute invalid."[76] The authority to remove a case, then, was triggered when a citizen alleged that the state court would not enforce a civil right.

Although the discriminatory action or inaction of the state might create a situation in which federal enforcement of civil rights was necessary, this was not the basis of federal jurisdiction. The third section of the Civil Rights Act conferred jurisdiction on the federal courts when the citizen was unable to enforce a civil right in the state courts. It was the citizen's lack of success that Senator Trumbull emphasized. He distinguished between cases in which state laws discriminated but the state court enforced the citizen's right anyway, and cases in which the laws did not discriminate, but, for some other reason, such as the racial or political prejudice of the judge or jury, the citizen was still unable to enforce the right in the state courts. Senator Trumbull asserted that Congress had authority over both: "If it be necessary in order to protect the freedman in his rights that he should have authority to go into the Federal courts in all cases where a custom prevails in a State, or where there is a Statute-law of the State discriminating against him, I think we have the authority to confer that jurisdiction."[77] Senator Trumbull was referring to invidious treatment of blacks, but protection against politically discriminatory customs was also essential to support white political allies in the South. If federal law merely prohibited racially discriminatory state laws, white Unionists would not have been afforded the protection under federal law the framers intended to provide them.[78]

The framers did not believe that federal protection was required in all cases involving discriminatory laws and customs. If they had, the Civil Rights Act would have been made applicable whenever the laws and customs of a community or a state discriminated. Senator Trumbull observed that a person was not necessarily "discriminated against because there may be a custom in the community discriminating against him, nor because the legislature may have passed a statute discriminating against him."[79] The Civil Rights Act would have invalidated the law

[76] *Congressional Globe*, 39th Cong., 1st sess., 1866, p. 1759, emphasis added.

[77] *Congressional Globe*, 39th Cong., 1st sess., 1866, p. 1759. For a contrary view, see Fairman, *Reconstruction and Reunion, 1864–88*, 1238–44.

[78] In addition to the obvious need to protect blacks and white Unionists from overt hostility, Section 3 also addresses more subtle forms, such as that described by Southern Unionists who petitioned congressional Republicans to confer jurisdiction on the federal courts to enforce their economic rights. Unionist creditors who rejected Confederate money in payment of debts risked having those debts cancelled by state statute and rendered unenforceable in state courts. They pleaded for access to the federal courts to enforce these debts and other contracts that they believed political prejudice prevented them from enforcing in the state courts. T. J. Gretlows to Lyman Trumbull, 8 and 19 January 1866, Trumbull Papers, Library of Congress; George A. Custer to Zachariah Chandler, 14 January 1866, Zachariah Chandler Papers, Library of Congress. A concern for economic rights could explain the U.S. Circuit Court's decision applying the Civil Rights Act to protect white butchers who claimed that their civil rights under the Act were violated when the state conferred monopolistic privileges on a slaughterhouse corporation. *Slaughterhouse Cases*, 15 F. Cas. 655.

[79] *Congressional Globe*, 39th Cong., 1st sess., 1866, p. 1759.

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or custom, and Trumbull presumed that a state court would so hold. Therefore, however naively, Trumbull assumed that individuals might still be able to enforce their rights in the state courts despite the presence of discriminatory laws and customs.

The framers expressly distinguished between racially discriminatory laws and the actions of individuals who violated civil rights and asserted their intention of protecting against both. Senator Trumbull stated that the Civil Rights Act conferred on the courts of the United States jurisdiction "over all persons committing offenses against the provisions of this act, and also over the cases of persons who are discriminated against by State laws or customs."[80] Senator Lane also understood the statute's scope to include the actions of individuals acting under "the power of local prejudice to override the laws of the country."[81] Both senators expected the federal military to aid the federal judicial process "whenever there is a combination of persons in any of the rebellious states so powerful that the marshals and civil officers in the ordinary course of judicial proceedings cannot execute the law."[82] This concept of federal action presaged the Enforcement Act of 1871 and the Ku Klux Klan Act of 1871, which were directed at this kind of lawlessness.

Civil rights were doubly secured under the first three sections of the Civil Rights Act of 1866. A private cause of action was created for black citizens unable to exercise civil rights under the same conditions as white citizens, and anyone who infringed a citizen's civil rights under pretext of law or custom was subject to prosecution and punishment in the federal courts. In addition, the federal courts were authorized to assume original jurisdiction of civil and criminal cases when citizens were unable to enforce their rights in the state courts. Congress thus applied national authority directly to private individuals as well as to public officials. The framers asserted that Congress possessed this authority because they assumed that civil rights were recognized by the Constitution as rights of U.S. citizens.

Federal judges and legal officers interpreted the Thirteenth Amendment, the Fourteenth Amendment, and the Civil Rights Act of 1866 as conferring a broad authority to enforce civil rights directly, irrespective of the presence of discriminatory state action and regardless of the source of the violation, because these rights were the natural rights that belonged to all free citizens of a free republic. Indeed, the notion that a national civil rights enforcement authority was merely a guarantee of racially impartial government action was not judicially recognized in the federal courts until the Supreme Court's decisions in the 1870s.[83]

The Supreme Court's decisions narrowing enforcement authority reflected the North's diminished interest in Reconstruction.[84] By the 1870s, the Republican

[80] *Congressional Globe*, 39th Cong., 1st sess., 1866, p. 475.
[81] *Congressional Globe*, 39th Cong., 1st sess., 1866, p. 603.
[82] *Congressional Globe*, 39th Cong., 1st sess., 1866, p. 605.
[83] *Slaughterhouse Cases*, 16 Wall. (83 U.S.) 36; *United States v. Cruikshank*, 2 Otto (92 U.S.) 542; Kaczorowski, *Politics of Judicial Interpretation*, 1–48, 117–97.
[84] For a contrary view, see Michael Les Benedict, "Preserving Federalism: Reconstruction and the Waite Court," *Supreme Court Review* (1978): 39.

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party, a coalition of diverse political groups, began to fragment further over the policies and corruption of Ulysses S. Grant's presidency.[85] One group split away during Grant's first administration to form a separate party, the Liberal Republicans. They turned against Grant's Southern policy and resisted congressional interference in the South. Liberal Republicans also fought to curtail the size and power of the national government and to return political authority to local government. Their objectives indicated their desire for a return to normality and their interest in increasing states' rights for the purpose of controlling monopolies and the railroads. Some of the leaders of civil rights enforcement joined this movement. Lyman Trumbull was one. Reflecting these new political pressures, he changed his views regarding a national authority to enforce civil rights. By 1871, he was insisting that the 1866 Civil Rights Act and the Fourteenth Amendment applied only to racially discriminatory state action, and that they guaranteed no more than racial equality in state-conferred rights and a prohibition against racially discriminatory state laws.[86] By July of 1873, the Grant administration also lost interest in Reconstruction and ended its policy of civil rights enforcement.[87] Except for a few minor episodes of federal involvement in Southern affairs, black and white Republicans in the South were left on their own. Insofar as the enforcement of civil rights was concerned, Reconstruction ended long before the Compromise of 1877.

AS THE REPUBLICAN FRAMERS UNDERSTOOD THEM, the Thirteenth and Fourteenth Amendments were constitutionally revolutionary. These amendments delegated to Congress the authority to render a radical change in the role of the national government in American life. Congress and the federal courts had not participated to any great extent before 1860 in guaranteeing the fundamental and personal rights of citizens. Republicans chose to protect these rights in 1866 by enacting the Civil Rights Act, which conferred on the federal courts jurisdiction over and responsibility for enforcing the personal rights of citizens directly when citizens could not do so in the traditional institutions, namely, the state and local courts.

This new role for national institutions involved radical changes in constitutional law. Fundamental rights were secured and enforced through state law before the Civil War, but, afterwards, the civil rights statutes made fundamental rights a matter of national jurisdiction. The fundamental rights of citizens were now defined as rights pertaining to U.S. citizenship and, as such, were recognized by the Constitution and laws of the United States. Although the states were expected to continue in their traditional function of securing civil rights, their authority was to be shared with Congress and the federal courts. Because federal law was supreme, Congress and the federal courts could supplant all state authority over personal rights. The framers' legal theory of citizenship and congressional authority over the rights of citizens held the potential of ending federalism and

[85] For an excellent political history of the period, see William Gillette, *Retreat from Reconstruction, 1869–1879* (Baton Rouge, La., 1979).
[86] *Congressional Globe*, 42nd Cong., 1st sess., 1871, pp. 575–76.
[87] Kaczorowski, *Politics of Judicial Interpretation*, 111.

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establishing a consolidated, unitary state. That the framers eschewed this extreme institutional arrangement should not deflect attention from the other ways in which civil rights amendments and laws of Reconstruction represented, to the framers and federal legal officers, a revolutionary constitutionalism and a new American federalism centered in national authority and national institutions.

In the 1870s, the Supreme Court rejected the revolutionary congressional Republican theory of constitutionalism and read into the Thirteenth and Fourteenth Amendments the theory of states' rights promoted by congressional Conservative Democrats. The Court explicitly rejected the broader theory of a congressional civil rights enforcement authority, precisely because it was revolutionary. The Supreme Court preserved a modified theory of state sovereignty, resurrected a theory of American federalism based on states' rights, and recognized primary authority over citizenship and civil rights as residing in the states. Although American law denied the right of secession, it adopted other important elements of the antebellum theory of constitutionalism. Congressional framers of the Fourteenth Amendment and the Civil Rights Act of 1866 may have thought they were reconstructing American government and basing it on a revolutionary constitutional foundation, but the Supreme Court decided against this revolutionary constitutionalism in a reactionary resurgence of states' rights that resulted in the virtual reenslavement of Southern black Americans.

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The Social Analysis of Economic History and Theory: Conjectures on Late Nineteenth-Century American Development

JAMES LIVINGSTON

> The social factors. . . are usually taken for granted by the inhabitants of a nation and often overlooked in explaining economic developments.
>
> John W. Kendrick (*Productivity Trends in the U.S.*, 1961)

UNTIL RECENTLY, HISTORIANS WRITING ABOUT THE late nineteenth-century United States cast their story as if the hopelessly outclassed "people" inevitably lost whatever battle they happened to be fighting against "big business" or the larger but no less irresistible forces of market integration and technological innovation. Richard Hofstadter, Samuel Hays, and Irwin Unger, for example, differed with Fred Shannon, John Hicks, and Matthew Josephson on the sources and rationality of the majority's resistance to modernity in the form of corporate capitalism, but all assumed that the outcome was never in doubt, indeed, that another outcome would have been somehow unnatural.[1] Over the last fifteen to twenty years, new historiographical approaches have so deeply undermined this consensus that to retrieve its substance would require exhumation, not resuscitation.

The most important of these approaches are the new labor history and the new economic history. The new labor historians have demonstrated that the working class was perfectly capable of rational collective action, of winning its battles against

I have presented different parts or versions of this article to the Department of History at Northwestern University, the Newberry Library Labor History Study Group, the Triangle Economic History Workshop, and the National Museum of American History. On each occasion, the discussion that followed helped me clarify my argument. I am indebted more immediately, however, to the ideas, comments, and criticisms of Carl P. Parrini, Martin J. Sklar, Patricia A. Rossi, Harold D. Woodman, Stanley L. Engerman, Richard Sylla, Stanley N. Katz, Keith Haynes, William B. Burr, Leon Fink, L. Moody Simms, Jr., Ray Michalowski, Richard Schneirov, Lyman L. Johnson, and Louis Galambos.

[1] See Richard Hofstadter, *The Age of Reform* (New York, 1955), chaps. 1–3; Samuel Hays, *The Response to Industrialism* (Chicago, 1957); Irwin Unger, *The Greenback Era* (New York, 1965); Fred Shannon, *The Farmer's Last Frontier* (New York, 1945); John Hicks, *The Populist Revolt* (Lincoln, Nebr., 1931); and Matthew Josephson, *The Robber Barons* (New York, 1938). The radical freemarketeers among economic historians still assume that another outcome would have been unnatural; their assumptions are now embodied in textbooks designed for undergraduate use: see Robert Higgs, *The Transformation of the American Economy, 1865–1914* (New York, 1971); Jonathan Hughes, *American Economic History* (Glenview, Ill., 1982); and Stanley Lebergott, *The Americans: An Economic Record* (New York, 1984).

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big business, and of making history in its own right.[2] Meanwhile, a few of the new economic historians have suggested that the late nineteenth century could not have been altogether pleasant for big business because prices, profits, per capita output, and labor productivity growth declined alarmingly from 1870 to 1896.[3] The silence that has settled on the border between these two fields is particularly unfortunate, since both sides evidently agree that, in the late nineteenth century, big business simply did not have the political, economic, or cultural authority customarily ascribed to it.

This unacknowledged agreement between the new labor and new economic historians emerges at a moment when departures in political and cultural history lend powerful support to their revisionist theses. It is probably fair to say that from the remains of the old consensus a new one is already emerging, one which suggests that the late nineteenth century may best be characterized in terms of political, economic, and cultural stalemate. In any case, the recent literature shows that capitalists could not and did not make their social power legitimate or authoritative in any sphere of life until the last decade of the century.[4]

This inadvertent consensus may be taken as an invitation to rewrite the history of the United States in the late nineteenth century. It suggests at the very least that

[2] The labor history I have in mind is best represented in Herbert Gutman, "The Workers' Search for Power," *The Gilded Age*, H. Wayne Morgan, ed. (Syracuse, N.Y., 1970), 31–53; Herbert Gutman, "Class, Status, and Community Power in Nineteenth-Century American Cities—Paterson, New Jersey: A Case Study," *The Age of Industrialism in America*, F. C. Jaher, ed. (New York, 1968), 263–87; Melvyn Dubofsky, *Industrialism and the American Worker 1865–1920* (New York, 1975); Melvyn Dubofsky, "The Origin of Western Working Class Radicalism, 1890–1905," *Labor History*, 7 (1966): 131–54; Katherine Stone, "The Origin of Job Structures in the Steel Industry," *Radical America*, 7 (1973): 19–64; David Montgomery, "Workers' Control of Machine Production in the Nineteenth Century," *Labor History*, 17 (1976): 485–509; David Montgomery, *Workers' Control in America* (New York, 1979); Daniel Walkowitz, *Worker City, Company Town* (Urbana, Ill., 1982); P. K. Edwards, *Strikes in the United States, 1881–1974* (New York, 1981); Irwin Yellowitz, "Skilled Workers and Mechanization: The Lasters," *Labor History*, 18 (1977): 197–213; Lawrence Goodwyn, *Democratic Promise: The Populist Moment in America* (New York, 1976); and Leon Fink, *Workingmen's Democracy* (Urbana, Ill., 1983).

[3] The new economic history that rediscovered the questions and concerns of those who wrote in the shadow of the Great Depression is best represented in Jeffrey G. Williamson's work, "Late Nineteenth-Century American Retardation: A Neo-Classical Analysis," *Journal of Economic History*, 33 (1973): 581–607, "Watersheds and Turning Points: Conjectures on the Long-Term Impact of Civil War Financing," *ibid.*, 34 (1974): 636–61, *Late Nineteenth-Century American Development: A General Equilibrium History* (New York, 1974). Compare Rendig Fels, *American Business Cycles, 1865–1897* (New York, 1959), and Bernard Weber and S. J. Handfield-Jones, "Variations in the Rate of Economic Growth in the U.S.A., 1869–1939," *Oxford Economic Papers*, n.s., 6 (1954): 101–32.

[4] The political and cultural stalemate is documented, if not explicitly claimed or argued, in Paul Kleppner, *The Third Electoral System, 1853–1892* (Chapel Hill, N.C., 1979); Samuel T. McSeveney, *The Politics of Depression* (New York, 1972), chap. 1; C. K. Yearley, *The Money Machines: The Breakdown and Reform of Governmental and Party Finance in the North, 1860–1920* (Albany, N.Y., 1970); Stephen Skowronek, *Building a New American State: The Expansion of National Administrative Capacities, 1877–1920* (New York, 1982), Part II; Morton Keller, *Affairs of State* (Cambridge, Mass., 1977); Paul Boyer, *Urban Masses and Moral Order in America, 1820–1920* (Cambridge, Mass., 1978), Part III; T. J. Jackson Lears, *No Place of Grace: Antimodernism and the Transformation of American Culture, 1880–1920* (New York, 1981); Neil Harris, "Introduction," *The Land of Contrasts, 1880–1901* (New York, 1970), 1–28; Alan Trachtenberg, *The Incorporation of America* (New York, 1982); and Christopher Lasch, "The Moral and Intellectual Rehabilitation of the Ruling Class," in *The World of Nations* (New York, 1973), 80–99. These departures owe a great deal to John Higham, "The Reorientation of American Culture in the 1890s," *The Origins of Modern Consciousness*, John Weiss, ed., (Detroit, Mich., 1965), 25–48; Robert Wiebe, *The Search for Order* (New York, 1967); Sidney Fine, *Laissez-Faire and the General Welfare State* (Ann Arbor, Mich., 1956); and E. Digby Baltzell, *The Protestant Establishment* (New York, 1964), chap. 5.

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the relation between market power, political power, and cultural authority was anything but linear between 1870 and 1900. But this insight raises a methodological problem that must be addressed before we accept the invitation to rewrite a part of U.S. history: how do we propose, describe, and test a historical relation between the political and economic events we can quantify and the social or cultural phenomena we cannot? To put it another way, how can we integrate the new economic and the new labor history?

One promising solution is to be found in economic theory, the discipline that, as Robert Fogel has suggested, is the source of the new economic history. Over the last twenty-five years, the lack of an adequate theory of distribution consistent with the principle of marginal productivity has driven many economists toward the post-Keynesian model that emerged from the Cambridge debate on neoclassical capital theory, from 1954 to 1970. The most striking and significant finding of this unfinished model—it is more a critique of neoclassical economics than a theory in its own right—reveals that, since no such thing as a "quantity of capital" can exist apart from a given rate of profit, one cannot deduce rates of profit and other elements of income distribution from that quantity, no matter how it is measured. Instead, economists must posit a real wage and a "rate of exploitation" of labor and then proceed to estimate incomes as if capital is a continuum of ownership, not a quantity of goods whose measurable value is determined by its prospective yield. It follows that the distribution of income depends fundamentally on the balance of social power between the active factors of production (capital and labor), and not, as neoclassical theory holds, on the marginal productivity of capital goods. At a higher level of argument, it also follows that economic events are explicable only by reference to the social relations within which they appear—by reference, that is, to historically specific contexts of production and exchange.[5]

The new economic history in the late 1960s rose on theoretical foundations that had been found inadequate to bear any appreciable load of quantitative data and measurement. It is only now, in the aftermath of the Cambridge debate, that we can see that the social and cultural context of economic change is not what the new economic historians have assumed it to be—an "exogenous factor" that can safely be ignored in building quantitative models of economic behavior and growth. We can see instead that Karl Marx, Thorstein Veblen, and Joseph Schumpeter were not so very wrong to claim that economic change and progress have social and cultural causes and to analyze economic history and theory on that assumption.

[5] See Robert Fogel, "The Reunification of Economic History with Economic Theory," *American Economic Review*, 53 (1965): 92–99. The Cambridge debate may be said to have begun with Joan Robinson, "The Production Function and the Theory of Capital," *Review of Economic Studies*, 21 (1953–54): 81–106, but the detonating event was the appearance of Piero Sraffa's *Production of Commodities by Means of Commodities* (Cambridge, 1960). The dissatisfaction with existing theories of distribution is evident in Sidney Weintraub, *An Approach to the Theory of Distribution* (Philadelphia, Pa., 1958), and elsewhere, even within the empire of the new economic history; see, for example, Tibor Scitovsky, "A Survey of Some Theories of Income Distribution," in *The Behavior of Income Shares: Selected Theoretical and Empirical Issues* (Princeton, N.J., 1964), 15–31, especially 22, and comment following at 31–50 by E. F. Denison and Franco Modigliani. The Cambridge debate is illuminated by G. C. Harcourt and N. F. Laing, eds., *Capital and Growth* (Baltimore, Md., 1971), and E. K. Hunt and J. G. Schwartz, eds., *A Critique of Economic Theory* (Baltimore, Md., 1972).

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TABLE 1

Growth in GNP Per Annum

1869–78 to 1874–83	5.58%
1874–83 to 1879–88	4.76%
1879–88 to 1884–93	3.68%
1884–93 to 1889–98	2.55

SOURCE: Robert E. Gallman, "Gross National Product in the United States, 1834–1909," *Output, Employment, and Productivity in the United States after 1800* (New York, 1966), Tables 2, 6 at pp. 9, 22.

If these new historiographical tendencies represent an invitation to rewrite U.S. history of the late nineteenth century, the post-Keynesian critique of neoclassical theory provides a good reason to accept the invitation: a rationale for an attempt to reintegrate the economic and the social dimensions of that history.

This attempt begins with three hypotheses. First, the relative but significant retardation of growth rates after 1870 was the economic consequence of a social stalemate. Second, renewed economic growth after 1896 was but one aspect of a broader shift in the balance of social power and cultural authority. Third, the American version of the marginalist revolution in economic theory was part of a larger, pragmatic effort to grasp economic retardation as a problem of distribution—a social problem—and thus to describe the conditions under which capital could be defined as a factor of production and a legitimate claimant to a share of national income. Together, these hypotheses constitute an argument about the emergence of corporate capitalism that may be read as an alternative to both the new economic history and the so-called organizational synthesis; they suggest that the innovation we know as the modern corporate system was an economic solution to a stubborn social impasse, not merely an administrative response to market integration or technological imperatives.[6]

RENEWED INTEREST IN THE PUZZLE OF late nineteenth-century economic retardation is largely the result of pioneering work by J. G. Williamson. In a series of publications that appeared in 1973–74, he argued that the parallel between European and American economic experience after 1870 is close enough that students of the United States can safely borrow the periodization of Europeanists accustomed to viewing the era as the "Great Depression." Williamson showed that output per capita and the growth of labor productivity declined "with a marked persistence from 1870 to the turn of the century," and that interest rates (nominal and real), commodity prices, and yields on capital fell just as persistently

[6] See Louis Galambos, "The Emerging Organizational Synthesis in Modern American History," *Business History Review*, 44 (1970): 279–90.

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throughout the same period. He derived his explanation for this general retardation from a straightforward but rigorous application of neoclassical growth theory: the relative decline after about 1873, he argued, was an inevitable product of the economy's adjustment to conditions generated by the Civil War, particularly to the extraordinary leap in the rate of gross savings between the 1850s and the 1870s. In other words, the rate of capital stock growth had to decline from the initially high levels induced by this "once-over savings rate change" to one approaching the constant rate of labor force growth.[7]

Williamson's resurrection of the Great Depression thesis has not gone unchallenged. His colleagues among the new economic historians are likely to suggest that he has created an analytical problem by capricious dating, or that he has ignored the effect of a declining rate of population growth on rates of economic growth. Many colleagues would dismiss his argument as a restatement of the myth invented in the late nineteenth century, by those in business, who, under the spell of "money illusion," complained constantly about declining prices as if they caused declining profits, because they confused the nominal value of income with real purchasing power.

The empirical contours of economic retardation need to be established before we can reexamine its causes and consequences. To begin with, the secular decline of growth rates between 1870 and 1900 is not a statistical apparition created by an arbitrary selection of dates. The depression of 1873–78 was, in some respects, more severe than that of 1893–97; yet the retardation in the growth of gross national product per annum between 1870 and 1899 is clearly documented in standard sources.[8] (See Table 1.) The decline in growth of GNP per capita, per worker, and per work-hour over the late nineteenth century is also clearly documented in the sources. (See Table 2.) Hence, the decline in the rate of population growth from 1870 to 1900 cannot begin to account for economic retardation over the same period. Finally, the claim that businesspeople confused deflation with depression, and so should be taken no more seriously than Williamson should, ignores two significant aspects of the relevant historical reality. First, the "money illusion" can account for late nineteenth-century complaints about falling profits and commercial depression only if output per worker—labor productivity—was increasing relative to money supply. As the accompanying tables show, that crucial condition was not met in the deflationary 1880s and 1890s. Second, business opinion on the question of profitability determines investment behavior and thus the broad pattern of economic growth in a market economy. Entrepreneurs and executives may well feel proud of their role in creating an abundance of commodities, but, in their own interest, they must be concerned more with the market-exchange value of commodities than with the use-value of commodities. Thus, they may define a period of unprecedented absolute growth in real output as a depression, because the market clears at prices that do not cover

[7] See Williamson, "Late Nineteenth-Century Retardation," and *Late Nineteenth-Century American Development*, chap. 5.

[8] See A. R. Eckler, "A Measure of the Severity of Depressions, 1873–1932," *Review of Economics and Statistics*, 15 (1933): 75–79.

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TABLE 2

Growth in GNP Per Annum

Decade	Per Capita	Per Work-Hour
1874*–84	3.8%	3.2%
1884**–94	0.6%	0.6%
1889–99	2.3%	2.3%

* 1869–78 decade average is centered on 1874.
** 1879–88 decade average is centered on 1884.

Overlapping Decades	Per Worker
1878–82 to 1888–92	1.0%
1883–87 to 1893–97	0.58%
1888–92 to 1898–02	1.50%
1893–97 to 1903–07	2.36%

SOURCES: U.S. Bureau of Economic Analysis, *Long-Term Growth, 1860–1970* (Washington, D.C., 1973): Part V, charts 18, 20 at pp. 107, 109; Simon Kuznets, "Notes on the Pattern of U.S. Economic Growth," in *Economic Growth and Structure* (New York, 1965): Table 1 at p. 305.

real costs or allow reasonable profits. In any case, we should not be surprised if businesspeople are not as eager to serve the public good as they are to preserve the incomes and discretion that accrue to private entrepreneurs in a market society.

Some economists and historians will accept the empirical contours of late nineteenth-century economic retardation as they are sketched here but will insist they are insignificant because the growth rate of the GNP per annum was higher then than it has been in the twentieth century. This line of argument fails to acknowledge, however, that the more inclusive measure of economic growth, GNP per capita, *increases* from the nineteenth to the twentieth century.[9] As Williamson emphasized, the question is not whether overall growth took place between 1870 and 1900—of course, it did—but how to explain variations in the *rate* of growth: the real questions are why the rate of growth fell between 1873 and 1896, and why it increased thereafter.

Williamson's neoclassical model served him well in explaining late nineteenth-century economic retardation, but he acknowledged that it cannot account for the "turn of the wheel" after 1896. This is not to suggest that a more-or-less Keynesian model has produced what is needed, a unitary explanation of the data on retardation and renewed growth. As Williamson pointed out, "None of the neo-Keynesians have offered an explicit test of the aggregate demand thesis."[10] Yet

[9] See Robert Gallman, "Gross National Product in the United States, 1834–1909," *Output, Employment, and Productivity in the United States after 1800* (New York, 1966), 8–10.
[10] See Williamson, *Late Nineteenth-Century American Development*, 117–18.

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several influential nineteenth-century economists developed and, in fact, tested a coherent explanation of economic retardation predicated on a concept of aggregate demand, which, accordingly, recognized no contradiction between a higher rate or volume of savings on the one hand and a "profit squeeze" or falling real interest rate on the other. In the terms of this model, the symptom, curse, and perhaps the cause of late nineteenth-century economic retardation was a shift in the distribution of income from profits to wages, or from capital to labor, a shift that was enforced by patterns of investment, production, and employment peculiar to the postwar era. From the standpoint of these economists, therefore, the condition of a faster rate of economic growth was a means by which capital could reclaim the income transferred from profits to wages after 1873.

The economists who developed this model in the late 1880s and early 1890s were David Wells, Edward Atkinson, Carroll D. Wright, Jeremiah Jenks, and Charles Conant. Each emphasized real, not monetary, factors in explaining what Wells, Atkinson, and Jenks, for example, defined as a prolonged depression that began in the mid-1870s. All of them suggested that inadequacies of aggregate demand were at the heart of the economic problem. More to the point, each of these observers used statistical evidence to argue that the consequence of late nineteenth-century depression was a shift in the distribution of income between capital and labor, which, by the late 1880s and early 1890s, had perhaps become a cause of economic retardation. Their calculations showing labor's gain at the expense of capital were designed, then, not as a defense of the *status quo* but as proof of the need to restate and reconstitute the conditions for capital's claim to a reasonable share of national income.

For example, in Wells's landmark study, *Recent Economic Changes*, he noted that "there may be . . . an amount of production in excess of demand at remunerative prices, or, what is substantially the same thing, an excess of capacity for production." He suggested, moreover, that this theoretical possibility of overproduction had become a depressing actuality after 1873. The consequences of production beyond effective or remunerative demand were, first, a general decline of prices, and second, a shift in the distribution of income from profits to wages. "If there is a progressive fall of prices without a corresponding fall of wages," Wells argued, "profits must fall progressively, and interest also, since the rate of interest is governed by the profits which can be made from the use of the capital. Now this is exactly what has happened in recent years. Profits and prices of commodities have fallen, except in a few special departments. Consequently the purchasing power of wages has risen, and this has given to the wage-earning class a greater command over the necessaries and comforts of life."[11]

[11] David Wells, *Recent Economic Changes* (New York, 1889), 84–86, 406–22. This is probably the most cited book of the period 1890–1910. That Wells and the others did not see the tendency of economic development any differently than their counterparts in the late nineteenth-century business community is documented in Willard Thorp's *Business Annals* (New York, 1926), 103, 131–37. As a theoretical proposition, Wells's argument here is questionable because it does not take productivity increases into account. But he was more interested in the empirical contours of economic change after 1873 and indeed refused to integrate his evidence on overproduction into a theory of business cycles.

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Jeremiah Jenks, an economist at Cornell University who became counsel to the U.S. Industrial Commission in 1900, agreed with Wells in every important respect. In a review of *Recent Economic Changes*, Jenks observed that "the business depression of the last fifteen years" was a development worthy of the "careful study" Wells had devoted to it. And he, too, suggested that "the capitalists [had] suffered[ed] most, proportionally," in this prolonged depression because the "remarkable reduction in prices" that characterized the era was the consequence of a "large increase" of output that, in the absence of remunerative demand, meant "small profits." By contrast, Jenks emphasized, "Wages, on the whole, have steadily risen."[12]

Edward Atkinson, a Boston textile manufacturer and economist, also argued that the late nineteenth century was not a promising epoch for capital: "Since 1873 a great and general reduction of prices has taken place the world over. What has been called depression has been more common than activity in commerce." His calculations showed that the purchasing power of skilled labor had risen about 100 percent since 1865, while the earning power of capital had declined "absolutely one half and relatively at least 75 percent since 1860."[13]

Carroll D. Wright, a distinguished commissioner of labor for Massachusetts and the U.S., was similarly interested in the distribution of national income and for the same reasons. He and Atkinson became consultants to Senator Nelson Aldrich's Finance Committee when it set out, in 1890, to chart the disturbing and possibly dangerous relation between prices and wages. Wright's sophisticated statistical studies (he used weighted averages to compute necessary expenditures for wage earners) showed that wages in industry rose almost 70 percent between 1860 and 1891, while the "prices of 223 commodities entering into consumption" fell about 6 percent over the same period.[14]

Charles Conant, the country's leading authority on banking and a prominent theoretician of banking reform from 1898 to 1913, summarized the effect of these tendencies in 1896: "Those laborers who continue to earn their customary wages are benefitted materially in a period of low prices, because of the greatly increased purchasing power of their earnings. An industrial enterprise which continues to operate without profit or at a loss during a depression . . . transfers all its benefits,

[12] Jeremiah Jenks, "The Economic Outlook," *The Dial*, 10 (1890): 252–54; compare Jenks's later and more comprehensive effort, *The Trust Problem* (New York, 1900). On his significance as a theorist and policy-maker, see Carl P. Parrini and Martin J. Sklar, "New Thinking about the Market, 1896–1904: Some American Economists on the Theory of Investment and Capital Surplus," *Journal of Economic History*, 43 (1983): 559–78.

[13] Edward Atkinson, *The Industrial Progress of the Nation* (New York, 1889), 111–21. That Atkinson's calculations with respect to the earning power of capital were not mere figments of a paranoid imagination is demonstrated in Paul Uselding, "Factor Substitution and Labor Productivity Growth in Manufacturing, 1839–1899," *Journal of Economic History*, 32 (1972): Table 1 at p. 672, col. 5.

[14] Carroll D. Wright, "Cheaper Living and the Rise of Wages," *Forum*, 16 (1893): 221–28, which is in part a review of *Retail Prices and Wages*, Report by Mr. Aldrich from the Committee on Finance, 19 July 1892, 52nd Congress, 1st sess., S. Report 896, 3 vols. (Washington, D.C., 1892). Compare *Wholesale Prices, Wages, and Transportation*, Report by Mr. Aldrich from the Committee on Finance, 3 March 1893, 52nd Congress, 2d sess., S. Report 1394 (Washington, D.C., 1893). See also Wright's *Industrial Evolution of the United States* (New York, 1895), 222–27, and, with respect to overproduction and the fall of prices, his *First Annual Report of the U.S. Commissioner of Labor* (Washington, D.C., 1886), 64–91, 254–63.

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therefore, to the wage earners, and their wealth is enhanced at the expense of the owners of inherited or accumulated capital." Since Conant, like Wells, Atkinson, Jenks, and Wright, equated civilization and progress with private investment and capital accumulation, the problem of income distribution so conceived involved more than equity in the short run.[15]

All these observers, it should be emphasized, were aware that economic growth after 1870 was, in absolute terms, unprecedented. Wells pointed out that, in the quarter-century after 1873, economic change was "more important and varied than during any former corresponding period of the world's history." The "greater control over the forces of nature" produced by this change was self-evident. The problem, again, lay in capital not receiving appropriate remuneration for its contribution to growth. By the 1890s, in other words, capitalists were in danger of becoming public servants, as E. S. Meade of the University of Pennsylvania suggested in 1900.[16]

Williamson has demonstrated that these observers had reasonable grounds for their concern about the character of economic growth in the late nineteenth century. But we need to know whether their more specific concern about a transfer of income from capital to labor was equally reasonable or is equally subject to verification by the use of modern techniques. Here, a post-Keynesian model becomes useful. It suggests that the key to income distribution between capital and labor is the "rate of exploitation." Assuming that we may translate and quantify this term as the rate of labor productivity growth, and that we can measure real wages over time, we can use the model to postulate changes in income distribution. The relation between growth in real wages and labor productivity is what concerns us, as Clarence D. Long explained in his monograph on late nineteenth-century wages and earnings: "So large is labor's share of national income that any substantial dispartity between productivity and real wages would exert great impact on the other shares—either largely expropriating them or presenting them with huge windfalls."[17] If real wages are increasing rapidly as a result of drastic price deflation, for example, and labor productivity is not increasing at a comparable rate, capital's share of national income will be to that extent "expropriated" by labor.

If we can judge from the data produced by the National Bureau of Economic Research (NBER) and other agencies, this hypothetical situation corresponds to

[15] Charles A. Conant, *A History of Modern Banks of Issue* (New York, 1896), 464. There is no biography of Conant. His critical importance is suggested, however, in David Healy, *U.S. Expansionism* (Madison, Wisc., 1970), 194–209; Parrini and Sklar, "New Thinking about the Market"; and in my *Origins of the Federal Reserve System: Money, Class, and Corporate Capitalism 1890–1913* (Ithaca, N.Y., 1986), chaps. 3–7.

[16] Wells, *Recent Economic Changes*, v–vi; E. S. Meade, "Financial Aspects of the Trust Problem," *Annals of the American Academy of Political and Social Science*, 12 (1900): 10–12.

[17] See Weintraub, *Approach to Income Distribution*, 53–54 (Weintraub uses a one-factor model, that is, he takes labor productivity to be the crucial variable); Clarence R. Long, *Wages and Earnings in the United States, 1860–1890* (Princeton, N.J., 1960), 112; compare a mathematical version of the argument in A. L. Marty, "Diminishing Returns and the Relative Share of Labor," *Quarterly Journal of Economics*, 67 (1953): 615–18. Martin Bronfenbrenner deals with some of the related and larger issues of income distribution in "Some Neglected Implications of Secular Inflation," *Post-Keynesian Economics*, K. Kurihara, ed. (New Brunswick, N.J., 1954), 31–58.

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historical reality in the United States of the late nineteenth century. Price deflation and the consequent growth of real wages were not offset, it seems, by a comparable growth of labor productivity. But the disparity between growth in real wages and labor productivity is most noticeable in the later stages of the period, between 1884 and 1894. Productivity in the non-farm sector barely improved in these ten years, while real wages continued to increase rapidly, at a rate five to six times faster than productivity. Meanwhile, output per industrial worker calculated in dollar amounts declined 5 percent from 1874–83 to 1884–93, as earnings per industrial worker increased about 4 percent.[18]

This divergence is striking enough in itself, given what we know about absolute rates of growth in the period as a whole. But the 1880s also saw a doubling of the capital endowment per worker and the beginnings of a movement in industry toward the rationalization of work routines and cost control through "systematic management."[19] The growth of labor productivity did not suffer for lack of effort by capitalists and their representatives on the shop floor. Thus, concern about a transfer of income from capital to labor was becoming eminently reasonable by 1890; it is no accident that, between 1888 and 1896, many influential interpreters of nineteenth-century economic development began to worry about the implications of such a shift in the distribution of income.

In retrospect, the solutions to the problem of economic retardation seem obvious. On the supply side, capitalists had to reduce wages, speed up the mechanization of production, and, ultimately, seek to adjust output more nearly to demand, if they hoped to bring growth of real wages and labor productivity into line. On the demand side, their only hope—at least for the time being—was in foreign markets, because price stabilization was impossible without control of domestic supply. This need probably explains why empire was an inviting prospect in the 1890s. But the development of foreign markets was practically impossible without the economies of scale promised in large corporate enterprise, a conclusion that nineteenth-century analysts of the business system did not fail to draw.[20] In this sense, every possible solution to the problem of retardation as

[18] See John W. Kendrick, *Productivity Trends in the U.S.* (Princeton, N.J., 1961), Tables A–20, 21, 23, at pp. 331–32, 338; Stanley Lebergott, *Manpower in Economic Growth* (New York, 1964), 524, 528; U.S. Bureau of Economic Analysis, *Long-Term Growth, 1860–1970* (Washington, D.C., 1973), Part V, charts 18, 20, at pp. 107, 109; Weber and Handfield-Jones, "Variations in the Rate of Economic Growth," figures 1, 7, at pp. 104, 121; and E. C. Budd, "Factor Shares, 1850–1910," *Trends in the American Economy in the Nineteenth Century* (Princeton, N.J., 1960), Tables C–4, 5 at p. 398. See also the estimates of real wages and income shares, circa 1850–1900, in W. I. King, *The Wealth and Income of the People of the U.S.* (New York, 1915), 158–207, and Charles B. Spahr, *An Essay on the Present Distribution of Wealth in the United States* (New York, 1896), 95–109.

[19] See Long, *Wages and Earnings*, 61–64; Daniel Nelson, *Workers and Managers* (Madison, Wisc., 1975), 34–54; Alfred D. Chandler, *The Visible Hand* (Cambridge, Mass., 1979), 271–75; Joseph Litterer, "Systematic Management: The Search for Order and Integration," *Business History Review*, 35 (1961): 461–76; and Leland H. Jenks, "Early Phases of the Management Movement," *Administrative Science Quarterly*, 5 (1960): 421–47.

[20] See Walter LaFeber, *The New Empire* (Ithaca, N.Y., 1963); chaps. 2–4; Thomas McCormick, *China Market* (Chicago, 1962), especially chap. 1. For examples of the connection between orderly overseas marketing and corporate control of supply, see J. Jenks, *The Trust Problem*, 112; Charles Conant, *The U.S. in the Orient* (New York, 1901); J. B. Clark's remarks in *Chicago Conference on Trusts* (Chicago, 1899), 407.

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defined by Wells and the others presupposed capital's effective control of the supply side. As a practical question, effective control meant control of that realm of social life we call the labor process. The key was to accelerate the pace of mechanization at the point of production, because that meant abolishing the high labor costs associated with craft skill.[21]

If we can judge from the work of the new labor historians, this acceleration is precisely what capitalists could not accomplish in the late nineteenth century. Until the mid-to-late 1890s, skilled workers were able to enforce social norms that sanctioned their control of machine production. Before then, the extent of the division of labor and the pace of mechanization in the factory were largely determined, it seems, not by management's new rules of efficiency and reasonable profitability, but by work rules first enacted spontaneously by skilled trades, then codified in union contracts, and ultimately enforced by strikes. The official historian of the Carnegie Homestead Works complained, for example, that, until the skilled workers' union was destroyed by a lockout in 1892, the "method of apportioning the work, of regulating the turns, of altering the machinery, in short, every detail of working the great plant, was subject to the interference of some busybody representing the Amalgamated Association." Extraordinary as it might sound, this complaint was neither unfounded nor confined to the steel industry.[22]

John Frey, an experienced iron molder and a leading labor journalist, ascribed the considerable power exercised by skilled workers at the point of production to the indivisibility of mental and manual labor that workers maintained, first as an unspoken cultural tradition and, then, more consciously, through union work-rules: "It is this unique *possession* of craft knowledge and craft skill on the part of a body of wage workers, that is, their *possession* of these things and the employers' ignorance of them, that has enabled the workers to organize and force better terms from the employers." But, as Frey recognized, the mechanization of industrial production threatened to remedy the ignorance of employers because it entailed a "separation of craft knowledge from craft skill"—that is, it promised to reconstitute and systematize in specialized machines the "scattered craft knowledge" of skilled workers, leaving them with only their "manual skill and dexterity" to sell in the labor market. "The machinery instead of the man is the brains," as a young machinist explained the result to a Senate committee in 1883.[23]

[21] On the cost-generating character of craft skill, see Nathan Rosenberg, *Technology and American Economic Growth* (New York, 1972), chap. 4, especially pp. 94–95; for a particular example of the cost reductions made possible by machine production, see P. G. Porter, "Origins of the American Tobacco Company," *Business History Review*, 43 (1969): 59–76, especially 69. See also the treatment of technology, skill, and income in J. G. Williamson and Peter H. Lindert, *American Inequality: A Macroeconomic History* (New York, 1980), 143–46.

[22] In addition to the articles by Stone, Dubofsky, Montgomery, and Yellowitz cited in note 2, see John Laslett, *Labor and the Left: 1880–1924* (New York, 1970), 152–59; Robert Ozanne, *A Century of Labor-Management Relations at McCormick and International Harvester* (Madison, Wisc., 1967), 16–28; John Fitch, *The Steel Workers* (New York, 1911), 99–104; and Joseph F. Wall, *Andrew Carnegie* (New York, 1970), 528–40.

[23] John Frey is quoted at length in Robert Hoxie, *Scientific Management and Labor* (New York, 1915), 132–37, emphasis in original. The machinist, John Morrison of New York, is quoted from his testimony before the Senate Committee on Education and Labor, *Report of the Committee of the Senate upon Relations between Capital and Labor and Testimony taken by the Committee*, 48th Congress, 2d sess., S. Report 1262

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TABLE 3

Issues in Strikes, 1881–1905

	Strikes over Working Conditions* Including Hours Excluding Wages	Strikes over Working Conditions Excluding Union Recognition
1881–85	30.6%	22.8%
1886–90	51.1%	37.1%
1891–95	49.7%	34.3%
1896–1900	50.2%	34.3%
1901–1905	60.7%	31.6%

* working conditions = hours, union recognition and rules, employment of certain persons, method and time of payment, work rules, discipline, etc.

SOURCE: Adapted from P. K. Edwards, *Strikes in the United States, 1881–1974* (New York, 1981), Table 4.3 at p. 92.

It was this fundamental division of mental and manual labor, not machine production or "industrialism" as such, that skilled workers and their allies fought with remarkable tenacity in the 1880s and 1890s. There are three ways to measure their success. First, wages were not reduced to the extent that managers wanted or sought between 1886 and 1894, largely because longer hours could not be imposed on workers. Second, the gross surplus available to manufacturers—the share of revenue from value added that industrial capitalists could retain after covering wages—declined noticeably over the same years.[24] Third, the growth of productivity virtually ceased until 1895–96. But skilled workers were successful in these terms because they did not have to rely on the resources they could bring to bear at the point of production. They did not ignore or misuse their ultimate weapon, the strike—in fact, they struck more often when the battle over control of the workplace intensified in the late 1880s and 1890s.[25] (See Tables 3 and 4.) Even so, skilled workers could not have been as effective as they were if their identification with and access to the sources of political power and cultural

(Washington, D.C., 1885), vol. 1: 763. From the standpoint of skilled labor, scientific management thus signified more than Frederick Winslow Taylor's compulsive clockwork. To claim that scientific management was relatively unimportant because Taylor's disciples were never able to transform industrial production according to their particular principles is, then, to miss the point.

[24] See David M. Gordon, *et al.*, *Segmented Work, Divided Workers: The Historical Transformation of Labor in the United States* (New York, 1982), 97–98, 147, and n. 29 at 257–58.

[25] See especially Edwards, *Strikes in the U.S.*, 84–114; Montgomery, "Workers' Control," Table 1; David Montgomery, "Strikes in Nineteenth-Century America," *Social Science History*, 4 (1980): 81–104, especially 88–93; and the *Twenty-First Annual Report of the U.S. Commissioner of Labor: Strikes and Lockouts, 1881–1905* (Washington, D.C., 1906), 56–64, 70–77.

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TABLE 4

Strikes and Their Resolution, 1881–1905

	Number		*Percent by Labor*		
Years	Strikes	Establishments	Won	Compromised	Lost
1881–85	2,491	12,443	56.1	8.7	35.2
1886–89	4,849	27,270	44.2	13.1	42.8
1890–93	6,153	27,635	43.7	9.8	46.5
1894–97	4,668	29,123	53.0	15.1	31.9
1898–1901	7,556	35,282	54.4	15.0	30.6
1902–05	11,040	52,990	38.5	19.1	42.4

SOURCES: *Twenty-First Annual Report of the U.S. Commissioner of Labor: Strikes and Lockouts, 1881–1905* (Washington, D.C., 1906), 15; Edwards, *Strikes*, Table 2.6 at p. 42.

authority had not protected and enlarged the social meaning of their strikes. For example, external forces of "law and order" were usually neutralized during the strikes of the 1880s, since many local officeholders, editors, and shopkeepers supported striking workers (skilled and unskilled) against the large employers. This solidarity was possible partly because the market power of large capital had not yet been acknowledged as a permanent or legitimate dimension of American society. This situation made strikebreaking and union-busting more difficult and, to that degree, upheld skilled workers' claims at the point of production.[26]

In the 1880s and, to a lesser extent, the 1890s, skilled workers were able to enforce, in contracts and by strikes, those social norms that presumed and sanctioned their control of machine production. In short, they won the battle against "systematic management." They lost the war, of course, as both the new and old labor historians have demonstrated. But the number, pattern, and resolution of strikes in the 1880s and early 1890s suggest that, by and large, skilled workers were successfully meeting the challenges of employers to effective control

[26] See Herbert Gutman's work, "Class, Status, and Community Power," especially 269 and following; "Protestantism and the American Labor Movement: The Christian Spirit in the Gilded Age," *AHR*, 72 (1966): 74–101; "Work, Culture, and Society in Industrializing America," *AHR*, 78 (1973): 531–88, especially 567–71; "Trouble on the Railroads in 1873–74: Prelude to the 1877 Crisis?"; and "Two Lockouts in Pennsylvania, 1873–74," in *Work, Culture, and Society in Industrializing America* (New York, 1976), 295–343, especially 303, 307–12, 340–41. See also Dubofsky, *Industrialism and the American Worker*, 34–41; Walkowitz, *Worker City, Company Town*, 192, and following; Ozanne, *Century of Relations*, 26–28; Fink, *Workingmen's Democracy*, 25–33, 50–56, 119–26, 221–23; and Michael J. Cassity, "Modernization and Social Crisis: The Knights of Labor and a Midwest Community, 1885–86," *Journal of American History*, 66 (1979): 41–61, especially 43–47. The remarkable solidarity of skilled and unskilled workers may also be inferred from the anomalous ratios of skilled to unskilled workers' wages in manufacturing and building trades, from 1884 to 1899: at the moment of economic development when inequality should have been rising and pay ratios should have been widening, these pay ratios were doing no such thing. See Williamson and Lindert, *American Inequality*, 71–82, 163–64, and Appendix D at p. 307.

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of machine production and were thus enforcing the disparity between productivity and real wages that transferred income from capital to labor. Skilled workers, by maintaining their control of machine production, may also have slowed the rate of "technological convergence"—the rate at which new metal-working techniques are exported from machine tool industries and adapted to wider uses in the capital goods sector—and thus may have prevented the kind of cost reductions, improved capital efficiency, and increased productivity that technological innovation in this sense often promotes.[27] From either standpoint, late nineteenth-century economic retardation derived from a fundamental social stalemate.

THE ARGUMENT ACQUIRES MORE PLAUSIBILITY IF THE TURN OF THE WHEEL after 1896 can be explained in similar terms, that is, as the product of a shift in the balance of social power. To begin with, we need to know how the existing relation between growth in real wages and labor productivity was reversed after 1894, so that capital's share of national income grew relative to labor's share. The pivotal question concerns labor productivity, for, as we have seen, a real wage increase does not necessarily entail a reduction of profits and a transfer of income from capital to labor.

An accelerated pace of mechanization in industry certainly contributed to the acceleration in the growth of labor productivity. But, as John W. Kendrick emphasized, the forces underlying growth in productivity are more complex than mere changes in technology. By his account, significant technological changes presuppose a "socio-economic organization or 'institutional' framework that enables or promotes the pursuit of efficiency."[28] We should, then, be able to describe a connection between change in this respect and increased labor productivity. One way to do so is to posit the industrial corporations that emerged from the crisis of the 1890s as a new "institutional framework" through which capitalists could assert their control of an increasingly mechanized labor process.

There are, in turn, two ways to frame the hypothesis. On the one hand, we can ask whether the new industrial corporations were in fact the instrument through which the social power of skilled workers was challenged and broken. On the other, we can ask whether the corporate innovators perceived the corporations they formed as an instrument for this type of social renovation. In historiographical terms, the approach through perception is the more interesting, because it may provide the outline of an alternative to the so-called organizational synthesis;

[27] See the citations at note 25 and David Montgomery, "Labor in the Industrial Era," *The American Worker*, Richard B. Morris, ed. (Washington, D.C., 1977), 127. The "old" labor history concentrated on the losing battle against mechanization under management control; see, for example, Selig Perlman and Philip Taft, *Labor Movements 1896–1932*, 112–16, vol. 4 of J. R. Commons, *et al.*, *History of Labor in the U.S.* (New York, 1935). On the question of "technological convergence" and its economic significance, see Nathan Rosenberg, "Technological Change in the Machine Tool Industry, 1840–1910," *Journal of Economic History*, 23 (1963): 414–43.

[28] Kendrick, *Productivity Trends in the U.S.*, 177. No matter whose estimates of real wage growth are accepted, after 1895, such growth declined to at least half the rate of the period 1884–1894. Moreover, there is little controversy on the question of labor productivity growth in the period 1895–1914. On real wages, compare Paul A. Douglas, *Real Wages in the U.S. 1890–1926* (New York, 1930), and Albert Rees, *Real Wages in Manufacturing 1890–1914* (Princeton, N.J., 1961).

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besides, there can be little doubt about the coincidence of the emergence of the large corporations, the mechanization and homogenization of the labor process, and the eclipse of the control of production by skilled workers from 1894 to 1919.[29]

Before we take up this approach, however, it should be noted that, in the late nineteenth century, the discussion of the "social question" normally implied or simply meant discussion of the "labor problem." The assumption that industrialization in the United States was producing a permanent working class and a class society according to the European model informed almost every contemporary analysis of economic development, regardless of the analyst's political sympathies. By the last decade of the century, equating the social question and the economic question had become typical. For capitalists as well as Populists, the reorganization or redefinition of the market economy became the only basis of social stability, progress, and justice.[30]

From the standpoint of the capitalists, coming to terms with the labor problem meant solving the problem of overproduction. As long as the determination of prices was left to the demand side, profits could be raised or reinstated only by cutting wages and breaking unions. Bitter class conflict appeared to be the inevitable result. In any case, wage cuts and lockouts did not always succeed in their purpose; at least, they had not between 1884 and 1892. The vice-chairman of the Chicago Conference on Trusts explained the dilemma this way: "A large part of the friction that has existed between capital and labor, causing strikes, lockouts and riots, was the result, in part, of overproduction. The product was unloaded at a loss, the owners tried to recompense themselves by cutting the wages of their workmen." Carroll D. Wright analyzed the problem in similar terms: "If there are more goods in the market than there is any demand for, and goods must be made at a loss, and there is no advantage in the purchase of raw material, labor is the only elastic feature in the [cost of the] product." Arthur T. Lyman, the treasurer of the Lowell Manufacturing Company, was more succinct: "When profits disappear wages must fall."[31]

[29] See Richard C. Edwards, *Contested Terrain: The Transformation of the Workplace in the Twentieth Century* (New York, 1979), chaps. 2–4; Richard C. Edwards, "Stages in Corporate Stability and the Risks of Corporate Failure," *Journal of Economic History*, 35 (1975): 428–57; Gordon, *Segmented Work, Divided Workers*, 128–45, and citations therein. See also Harry Jerome, *Mechanization in Industry* (New York, 1934), 55, and following.

[30] This is why Woodrow Wilson could state in 1897 that "only currency reform can touch the cause of the present discontents." *Public Papers of Woodrow Wilson*, Ray S. Baker and William E. Dodd, eds., 3 vols. (New York, 1926), 2: 329, 354. Compare the preface to Conant's *History of Modern Banks of Issue*: "The issue of the present and of the immediate future . . . is the best means of developing the possibilities of individual and national life. This issue is essentially an economic one"; p. vi.

[31] Stephen P. Corliss, from *Chicago Conference on Trusts*, 137; Carroll D. Wright and Arthur T. Lyman, from *Report of the Senate Committee on Relations between Capital and Labor*, 1: 426, 450. Compare Charles R. Flint, "Industrial Consolidation: What They Have Accomplished for Capital and Labor," *North American Review*, 172 (1901): 673; James J. Hill, "Combinations and the Public," in *The Trust: Its Book*, James H. Bridge, ed. (New York, 1902), 101–02. See also U.S. Industrial Commission, *Preliminary Report* (Washington, D.C., 1901), the remarks of W. T. Graham, vice-president of American Tin Plate Co., 855; Daniel G. Reid, president of American Tin Plate Co., 866, 885–86; Francis L. Stetson, 977; Samuel Dodd, president of International Silver Co., 1055. In U.S. Industrial Commission, *Report on Trusts and Industrial Combinations* (Washington, D.C., 1901), see John Pitcairn, president of Pittsburgh Plate Glass Co., 227; Charles R. Flint, organizer and director of American Smelting and Refining Co.

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Business leaders and influential interpreters of economic development had the same answer to the problem of overproduction: "combination," a corporate consolidation of control over investment and production. They proposed to narrow the social basis of investment decisions by centralizing claims to productive assets through the legal device of corporations. For example, Jeremiah Jenks noted that the "waste of competition . . . which comes from the inability of adapting one's plants and output to the needs of the market . . . can be partly saved by combination of many manufacturing establishments in one industry under one management." On this, the economists and the capitalists were practically unanimous.[32] If prices could be stabilized at a level that allowed reasonable profits, because supply was adjusted to demand—if overproduction was to this extent abolished—then capitalists would not have to treat wages as the crucial variable in calculating the difference between costs and prices that equaled profits. Thus, resorting to class war would not represent the best alternative to operating at a loss. This corporate alternative presupposed a social agenda; for, if it were to succeed, the small or marginal entrepreneurs would have to be effectively disciplined or simply purged from the economic organism: they would have to give up their once-commanding role in resource allocation if overproduction were not to be resurrected from the supply side. William Jennings Bryan's "broader class of businessmen" became an endangered species by the mid-1890s. "They are the ones who have been caught between the upper and the nether mill stones," as James J. Hill of the Great Northern Railway put it, "they are the middlemen, and the small competitor who was unable to meet the larger concern in the open market."[33]

and U.S. Rubber Co., 91–92; E. R. Chapman, organizer of American Smelting and Refining Co., 109; George Gunton, editor of *Gunton's Magazine*, 632. In *Chicago Conference on Trusts*, see Robert S. Taylor, 72; F. B. Thurber, president of U.S. Export Association, 125–26, 131, 135; Paul Morton, vice-president of the Atchison, Topeka, and Santa Fe Railroad, 249–50; W. P. Potter, member of the Pennsylvania Bar, and part of the influential upperclass Potter family of Philadelphia, 300, 303–04; David Ross, secretary of the Illinois Bureau of Labor Statistics, 372; Charles D. Willard, 552; E. P. Ripley, president of the AT&SF Railroad, 555; S. A. Martin, president of Wilson College, 578–79; Emerson McMillin, New York banker, 617–18; and Charles J. Bonaparte, New York lawyer and Roosevelt's attorney general, 622. In *Report and Proceedings of the Joint Committee of the [New York] Senate and Assembly Appointed to Investigate Trusts* (Albany, N.Y., 1897), see H. O. Havemeyer, president of the American Sugar Refining Co., 121–22; John E. Searles, treasurer of the American Coffee Co. and director of American Sugar Refining Co., 438–40; Charles R. Flint, 456–61, 530–36; F. B. Thurber, president of the American Grocer Publishing Co., 926–35; E. B. Thomas, president of the Erie Railroad, 1017–20; Edwin R. Holden, vice-president of the Delaware, Lackawanna and Western Railroad, 1049.

[32] J. Jenks, *Trust Problem*, 34–36. The evidence on this agreement is overwhelming. See, for example, vols. 1 and 13 of the U.S. Industrial Commission *Reports*; *Chicago Conference on Trusts*; *Proceedings of the American Economic Association*, 9 (1900); and *Annals: Corporations and the Public Welfare*, 12 (1900). For a summary, see chap. 2 of my *Origins of the Federal Reserve System*. It is worth noting that in the period 1886–96 "cooperation" took on a new meaning; instead of describing the outlook of the labor movement (particularly the Knights of Labor) and the Populists, "cooperation" became a word that described the outlook of corporate innovators and of those economists who were critical of laissez-faire capitalism, and who accordingly looked for an alternative in more civilized, more centralized forms of control over the investment system. On this semantic evolution and its corollaries, see Dorothy Ross, "Socialism and American Liberalism: Academic Social Thought in the 1880s," *Perspectives in American History*, 11 (1977–78): 5–79, especially 23–44.

[33] Hill, "Combinations and the Public," 100. Compare Sidney Sherwood, "The Influence of the Trust in the Development of Undertaking Genius," *Proceedings of the American Economic Association*, 9 (1900): 163, and following.

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The social agenda announced in this corporate alternative to overproduction necessarily included more than an elimination of small-scale entrepreneurs. Control of supply under corporate auspices meant consolidating ownership of the separate companies and dispersed plants that had competed with each other for sales and profits within limited markets. It also meant establishing or consolidating managerial control at the point of production, where skilled workers held sway. In the view of the corporate innovators, bringing an end to "ruinous competition" was senseless unless the plants that remained in operation after a merger or reorganization could be made more efficient and productive. If wage rates were not flexible (as indeed they were not), the condition of greater efficiency and productivity was the further mechanization of the labor process. Charles R. Flint, a founder of the U.S. Rubber Company and perhaps the most outspoken promoter of corporate consolidation, explained how this condition could be met only within the new institutional framework of the corporate system: "The American workingman to-day earns higher wages than are paid in any other country. This condition has been made possible . . . because the American workingman produces more, and he produces more because he has been supplied with the most perfect system of labor-saving machinery on earth. To supply this machinery, large capital is necessary. The individual manufacturer, standing alone, is not in a position to perfect his machinery in the same measure as the consolidated enterprise."[34]

In these terms, the most important of the "economies of scale" to be realized by the large corporations was their capacity to finance greater investment in fixed capital while reducing the ratio of fixed costs to the value of total output. Of the "various economies" residing in the large scale of the corporations, Flint and his peers emphasized above all else that "centralized manufacture permits the largest use of special machinery."[35] The corporation thus constituted a means to the end of mechanizing, and controlling, the labor process. Skilled workers and capitalists agreed that technological innovation at the point of production was a preeminently social process that would determine whether employers or employees controlled the labor process. A survey of production methods in industry by the U.S. Commissioner of Labor in 1898 found, for example, that cutting leather parts for shoes and boots could be (and was) done by hand in about half the time it took by machine. The apparent anomaly bothered the commissioner enough to query one respondent. "When called to the attention of the manufacturer he stated that the time was correct, and that a smart cutter unhampered by 'union rules' could perform the work in the time specified." The point of installing the cutting machinery was, then, to shift control of output from labor to capital, even though,

[34] Flint, "Industrial Consolidations," 675.

[35] Flint, "The Business Situation in the U.S. and the Prospects for the Future," *North American Review*, 172 (1901): 385; compare Hill, "Combinations and the Public," 102–03; Azel F. Hatch, *Chicago Conference on Trusts*, 65–67; Clem Studebaker, *ibid.*, 575; H. R. Seager and Charles A. Gulick, *Trust and Corporation Problems* (New York, 1929), 75–76.

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as the commissioner's report noted, the "greatest efficiency is obtained under the primitive method."[36]

Labor unions had a role in the corporate dispensation, but they could not be allowed to exercise any meaningful control of machine production. If unions limited the use of new labor-saving machinery to preserve the indissolubility of craft skill and craft knowledge, the "economies of scale" promised in the mechanization of production could not be realized. This threat held special significance when corporate leaders contemplated the development of foreign markets, a field of enterprise in which the large corporations were indispensable. For example, Frank Vanderlip, the vice-president of New York's National City Bank, declared in 1903 that "controlling the world's industrial markets" was almost certainly the destiny of the United States. "The only serious obstacle in the way of that," he noted, "will be our labor organizations." Unions that sought to increase wages were perfectly acceptable. If they sought any role in determining rates of output, however, as they were accustomed to doing in the late nineteenth century, they would undermine American competitiveness in world markets: "We are surrounded by conditions that will permit us to pay two or three times as much wages as our foreign competitors and still meet successfully their competition, but we cannot, in addition to that handicap, of high wages, permit the workers to limit production and hope for a successful outcome in a world contest."[37]

The corporate innovators were ultimately able to abolish any formal control of output by skilled or unionized workers; they succeeded where the entrepreneurs had failed. The builders and promoters of the corporations explained the difference in social terms. Their new creations provided the "means to industrial managers to combat unfair demands on the part of labor organizations," as Vanderlip stated, "for they are better united and able to meet organization with organization." The great industrial combinations were, then, both cause and effect of a new social order, because the concentration of capitalists as well as capital was a natural result of the corporate movement. John E. Searles, a director of the American Sugar Refining Company from its founding in 1891, described the process this way: "Perhaps the greatest of all benefits in the centralization is the concentration of technical knowledge and ability of the people connected with the business . . . When the Trust was organized, these gentlemen were brought together and this technical knowledge and skill was concentrated and utilized for the common good." James B. Dill, a lawyer who drafted the New Jersey statute of 1889 under which four out of five corporations were chartered during the merger

[36] U.S. Industrial Commission, *Report on Trusts*, 92; *Thirteenth Annual Report of the U.S. Commissioner of Labor: Hand and Machine Labor*, 2 vols. (Washington, D.C., 1899), 1: 115.

[37] Frank A. Vanderlip to James Phelan, 30 January 1903, from the Frank A. Vanderlip Papers, Rare Book and Manuscript Library, Columbia University. Vanderlip established his reputation on the question of foreign markets with his *America's Commercial Invasion of Europe* (New York, 1902). His distinction between acceptable and unacceptable trade unionism may help resolve, or at least clarify, the debate between labor historians and the historians of corporate liberalism (especially James Weinstein, in *The Corporate Ideal in the Liberal State* [Boston, 1968]) on the opinion of business leaders about labor's role in the new corporate society.

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wave of 1898–1903, took a broader view: "Industrial combinations are producing a new class of financiers, a new order of corporation men."[38]

According to its proponents, the corporate alternative to overproduction and class war established or entailed a social agenda that included at least three items. These were, first, the demise of those small, freely competing entrepreneurs who constituted the nineteenth-century version of a middle class; second, the creation of a new relation between workers and the mechanical conditions of work, through which control of the labor process could be shifted to employers and the growth of labor productivity could be guaranteed; third, the emergence of a new class of capitalists, a "new order of corporation men." In each case, the corporation represented the institutional means to the kind of social change that would allow economic recovery. Insofar as it destroyed the social basis of overproduction—a surplus of entrepreneurs—the corporation gave capitalists the opportunity and the resources to deal with the labor problem as a social question at the point of production.

THE NEW LEADERS OF BUSINESS WERE FORGING, out of economic crisis, a new identity, a new concept of their role in a modern market society; in doing so, they were redefining American society. As many historians have shown, this social process was grudgingly acknowledged in the economics of Populism. But the work of economists who did not assume that capitalists were parasitic growths on the body politic has received rather less attention. We should ask if these economists were aware of the same social process and its economic consequences and, if so, whether that awareness would matter in the short or long run. A "social analysis" of economic theory can provide provisional answers, so long as it is assumed that economic theorists in the late nineteenth century recognized a world external to their systems and sought to comprehend that world systematically.

The mainstream of modern economic theory has broadened considerably in the last thirty years, but it is still dominated by commitment to the marginalist principles invented in the late nineteenth century by both Austrian and British economists. According to these principles, commodities had a certain value not because of what they cost in human labor—this is what the "classical" political economy had supposed—but because consumers found utility in them. The value of a commodity was greater or smaller according to the state of demand for it, not the quantity of labor-time necessary to produce it. Since less demand naturally followed from increased consumption of the same commodity, its utility would fall

[38] Vanderlip to Ferdinand Baumgarten, 8 August 1904, Vanderlip Papers. John E. Searles is quoted from his testimony in *Report and Proceedings of the Joint Committee of the [New York] Senate and Assembly*, 439; James B. Dill, "Industrials as an Investment for Small Capital," *Annals: Corporations and the Public Welfare*, 12 (1900): 115; compare Stuyvesant Fish's remarks to *Chicago Conference on Trusts*, p. 565, and Charles A. Conant, "The Function of the Banker," *Quarterly Journal of Economics*, 17 (1903): 476–91. On the proportion of corporations organized under the terms of New Jersey's statute of 1889, see Ralph Nelson, *Merger Movements in American Industry* (Princeton, N.J., 1956), 65–66.

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as its supply increased; the final or marginal unit demanded would determine, therefore, the utility—the value—of the entire supply.[39]

The proponents of marginalist theory assumed their first task was to discredit the labor theory of value, because they understood themselves to be shifting the focus of political economy from the sphere of production to the sphere of consumption. Their starting point was the "psychological relation between individuals and finished goods," not the social relations among individuals engaged in producing goods.[40] In this respect, the mere statement of marginalist principles implied that production and consumption were not aspects of the same activity, and so were not directly connected in practice or theory. By focusing their theory on the demand side, the marginalists were announcing that the problem of supply (or production) had been solved, that the critical issue of the times was distribution. But marginalism cannot, on these grounds, be reduced to an "ideologically motivated" attempt to conceal the dark truths of capitalist production. The economic events of the late nineteenth century had demonstrated to pro-capitalist economists and socialists alike that the problem of distribution was indeed the critical issue, in theory and practice. The empirical evidence suggested that supply simply did not create its own demand.[41]

The new theoretical puzzles framed by acknowledgment of this evidence were innumerable. For example, a model that defined demand as the central problem of economic theory and policy in effect proposed that the market might not be "self-regulating." It followed that the non-legal and non-deliberative forces of social control and harmony that had stabilized the "self-regulating" market would somehow have to be reconstituted through the law and formal institutions.[42] More important, however, marginalism's focus on the issues of demand and distribution represented, or implied the possibility of, a new orientation toward the working class: the habits, desires, and demands of the majority now became a proper object

[39] See Maurice Dobb, *Political Economy and Capitalism* (New York, 1945), chaps. 2–3, 5; Ronald Meek, *Economics and Ideology* (London, 1967), 51–74; T. W. Hutchison, *A Review of Economic Doctrines, 1870–1929* (Cambridge, 1953); and especially R. D. C. Black, *et al.*, eds., *The Marginal Revolution in Economics* (Durham, N.C., 1973).

[40] See, for example, W. Stanley Jevons, *The Theory of Political Economy* (London, 1870), vi–vii, 47, 51–63, 82, 159–60; the quote is from Meek, *Economics and Ideology*, 208. Note that Meek's formulation could be applied to the psychological character of modern culture, which was invented in the same period.

[41] See the citations of the French and British sources in Charles A. Conant, "The Struggle for Commercial Empire," in *U.S. in the Orient*, 61–90; compare Simon Patten's effort to demonstrate the significance of consumption as an economic problem in "The Scope of Political Economy, 11 *Yale Review*, 2 (1893): 264–87. On economic retardation in Europe, see Joseph Schumpeter, *Business Cycles*, 2 vols. (New York, 1939), 1: 333–41; Hans Rosenberg, "Political and Social Consequences of the Great Depression of 1873–1896 in Central Europe," *Economic History Review*, 12 (1943): 58–73; E. Phelps-Brown and S. J. Handfield-Jones, "The Climacteric of the 1890s," *Oxford Economic Papers*, n.s., 4 (1952): 279–89; David Landes, *The Unbound Prometheus* (Cambridge, Mass., 1969), 231–48; and P. A. Gourevitch, "International Trade, Domestic Coalitions, and Liberty: Comparative Responses to the Crisis of 1873–1896," *Journal of Interdisciplinary History*, 8 (1977): 281–313.

[42] See Warren J. Samuels, *The Classical Theory of Economic Policy* (Cleveland, Ohio, 1966); Karl Polanyi, *The Great Transformation* (Boston, 1957), chaps. 16–18. Henry C. Adams, *The State in Relation to Industrial Action*, (n.p., 1886) is perhaps the clearest statement of the problem, but see also Simon Patten, "The Political Significance of Recent Economic Theories," *Annals of the American Academy of Political and Social Science*, 32 (1908): 82–94.

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of high theory (and so, not incidentally, the discipline of economics became that much more policy-relevant, even in strictly political terms).

This new orientation meant that the marginalist revolution, even at its most abstruse extremes, was immediately conditioned by and deeply concerned with the social question—with the implications of a permanent proletariat whose organized power and avowed purposes were not necessarily consistent with the requirements of either profitable production or social stability. This was especially true in the United States, where, as C. D. W. Goodwin pointed out, an "intense preoccupation" with "urgent policy issues" characterized the marginalist vanguard, and where the labor problem was understood to be particularly acute.[43] Hence, a good way to demonstrate the meaning of marginalism as a social theory is to explore its emergence in this country.

By the 1920s, scholars generally agreed that the enduring contributions of the marginalist revolution in the New World were developed within the province of theory that attempted to explain the distribution of income between various claimants on or factors of production. The American marginalists were the first to articulate an elaborate concept of the capitalist's active function in commodity production and, from this, to propose theories of cost, profit, and interest that fit the new "subjective" bent of economic analysis.[44] American economists of the late nineteenth century seemed to be obsessed with meeting the challenge Henry Sidgwick laid down in 1879, in his review of Francis A. Walker's *Wages Question*: "While Professor Walker's argument gives a *coup de grâce* to the wages fund theory, it supplies no substitute for it; it leaves us with no theoretical determination whatever for the average proportions in which produce is divided between labor and capital." Walker himself established the pattern of response with "The Source of Business Profits" in 1887. He overthrew the empiricist dictum of the *Wages Question* ("what we need is not a nice theoretical classification, but a just and strong exhibition of the great groups of our modern industrial society") and offered a down payment on "a complete and consistent body of doctrine regarding the distribution of wealth."[45] The efforts of Walker's contemporaries and successors

[43] Goodwin, "Marginalism Moves to the New World," *Marginal Revolution*, 298–99; compare Dorothy Ross, "Socialism and American Liberalism"; Mary O. Furner, *Advocacy and Objectivity* (Lexington, Ky., 1975), 182, and following; Fine, *Laissez-Faire and the General Welfare State*, chap. 8.

[44] The importance of the American contribution to the marginalist revolution has been grossly underestimated. Since the theory of value propounded by marginalists was essentially a theory of demand, it required as a condition of its coherence a theory of income distribution—that is, some account of how demand was made effective. This is what the American marginalists provided. See Maurice Dobb, *Capitalist Enterprise and Social Progress* (London, 1926), Part I, especially chaps. 2–6; C. A. Tuttle, "The Entrepreneur Function in Economic Literature," *Journal of Political Economy*, 35 (1927): 501–21; and Joseph Schumpeter, *History of Economic Analysis* (New York, 1954), 893–98. The lineage of modern capital theory is traced to American origins in, for instance, L. L. Pasinetti, "Switches of Technique and the 'Rate of Return' in Capital Theory," *Economic Journal*, 79 (1969): 508–31; P. Garegnani, "Heterogeneous Capital, the Production Function and the Theory of Distribution," *Review of Economic Studies*, 37 (1970): 407–36; Joan Robinson, "The Meaning of Capital," in her *Contributions to Modern Economics* (London, 1978), 114–25. It should be noted here that the labor theory of value did not require a separate account of income distribution: see Lawrence R. Klein, "Theories of Effective Demand and Employment," *Marx and Modern Economics*, David Horowitz, ed. (London, 1968), 138–75.

[45] Henry Sidgwick is quoted by Francis A. Walker at the outset of "The Source of Business Profits," *Quarterly Journal of Economics*, 1 (1887): 265; the other quotations are from Francis A. Walker, *The Wages Question* (New York, 1879), 248, and "Source of Business Profits," 268.

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in the United States may be viewed as either further installments or final payments, according to whether or not one thinks Marx and Veblen have to be brought into the bargain.

In 1903, J. H. Hollander of Johns Hopkins University tried to explain his generation's obsession with distribution. He suggested that Walker's attempt at a complete theory of distribution was "a reaction born of intimate acquaintance with American economic conditions from the traditional doctrines of the English classical political economy." Walker's acquaintance with those economic conditions was gained at first hand. For example, he served, along with Atkinson and Wright, as a consultant to the Senate Finance Committee during its investigation of wages and prices in the early 1890s. The most significant of the conditions Walker discovered appeared to be that capitalists had become, or were fast becoming, public servants. As Hollander noted, it seemed in the United States that "the laborer, and not the entrepreneur, received, or might receive if he asserted his claim thereto, whatever economic surplus or unearned increment resulted from industry."[46] For late nineteenth-century economists, the question that followed was, on what grounds could entrepreneurs legitimate their claims to a share of a national income, assuming it was possible for wage earners to receive "whatever economic surplus" results from industrial production. The answers Walker and his successors provided were predicated on a theory of "marginal productivity," which hypothesized that measuring the product of capital assets was analogous to measuring the rent of land.[47] The theoretical question they addressed in this manner was essentially the same as that which Wells, Atkinson, Wright, Jenks, and Conant understood to be a pressing practical question best conceived in historical and statistical terms. As Joseph Schumpeter realized, Sidgwick's challenge—the challenge for late nineteenth-century economic theory and practice—was "to define capital in a way that would qualify it to stand, in production and distribution, on a par with the labor factor and the land factor."[48]

Schumpeter dismissed the possibility that this challenge was political: the marginalists had a "strong analytic interest" in so defining capital, but it was "ridiculous," he insisted, "to speak of a political one." Most historians of economic thought would probably agree, mainly because they normally see progress or change in modern theory as the product of debate over technical puzzles within a professional community of scholars or scientists.[49] Yet Walker and his most

[46] J. H. Hollander, "The Residual Claimant Theory of Distribution," *Quarterly Journal of Economics*, 17 (1903): 271, 270; compare Simon Patten, "The Conflict Theory of Distribution," *Yale Review*, 17 (1908): 156–84.

[47] See Walker, "Source of Business Profits," 277–78; J. B. Clark, *The Distribution of Wealth* (New York, 1899), 53, 83–84, 127–35, 169–71, 180–84, 334, 356–66; Arthur Hadley, *Economics* (New York, 1896), 264–89. See Garegnani, "Heterogeneous Capital," sect. 5, and Maurice Dobb, "The Sraffa System and Critique of the Neo-Classical Theory of Distribution," in Hunt and Schwartz, *A Critique of Economic Theory*, 205–21, on the nature of the assumptions and results of marginal productivity theory as it has been used to explain the rate of profit and the broader distribution of income.

[48] Schumpeter, *History of Economic Analysis*, 899.

[49] Schumpeter, *History of Economic Analysis*, 899. Compare T. W. Hutchison, *On Revolution and Progress in Economic Knowledge* (Cambridge, 1978); M. Blaug, *Economic Theory in Retrospect* (Homewood, Ill., 1962), chaps. 8–11, especially 277–83; and A. W. Coats, "The Economic and Social Context of the Marginal Revolution of the 1870s," in *Marginal Revolution in Economics*, 37–58.

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celebrated successor, J. B. Clark, hoped that their new theories of distribution would have broad political consequences. Walker concluded his essay on the sources of profits with the following remarks: "The bearing of this view of the source of business profits upon the socialist assumption that profits are but unpaid wages is too manifest to require exposition. That this view of business profits, if fully understood and accepted by the wages class, would have a truly reconciling influence upon the always strained and often hostile relations between employer and employed, cannot be doubted."[50] In the opening chapter of his opus, *The Distribution of Wealth*, Clark announced, "To each agent a distinguishable share in production, and to each a corresponding reward—such is the natural law of distribution. This thesis we have to prove; and more hinges on the truth of it than any introductory words can state. The right of society to exist in its present form, and the probability that it will continue so to exist, are at stake . . . The welfare of the laboring classes depends on whether they get much or little; but their attitude toward other classes—and therefore, the stability of the social state—depends chiefly on the question, whether the amount they get be it large or small, is what they produce. If they create a small amount of wealth and get the whole of it, they may not seek to revolutionize society; but if it were to appear that they produce ample amount and get only a part of it, many of them would become revolutionists, and all would have the right to do so."[51] The original American marginalists did not compartmentalize, and they apparently did not think it was possible or appropriate to separate, their "analytic" and their "political" interests in the new theories of capital and distribution. In their view, the theoretical and practical meanings of marginalism were integrally related.

From the standpoint of the late twentieth century, this faith in the healing power of economic theory may seem slightly embarrassing. At least it was to Lionel Charles Robbins: "It has sometimes been argued—J. B. Clark is perhaps the chief culprit—that a proof that, under competitive conditions, productive agents are paid according to the value of their marginal physical product is a proof that such a society is just. This of course is a complete *non sequitur*."[52] Walker, Clark, and their allies in the marginalist vanguard apparently did not see it that way. Before we assume, as Robbins did, that the "scientific" content of marginal productivity can be removed intact from its original "ideological" form, we should ask why its American inventors did not notice the logical contradiction in which they had seemingly implicated themselves and their students.

In effect, the question is why the marginalists in the United States saw the need for a new language or a new market model of political obligation—why they felt they had to specify new conditions of mutuality, harmony, or equality among the competing interests that made up civil society. To begin with, the American

[50] Walker, "Source of Profits," 288.
[51] Clark, *Distribution of Wealth*, 3–4.
[52] Lionel Charles Robbins, *The Evolution of Modern Economic Theory* (London, 1970), 19. Compare Frank H. Knight, *Risk, Uncertainty and Profit* (1921; rpt. edn., New York, 1957), 109–14. This fact-value or science-ideology distinction informs Schumpeter's *History of Economic Analysis* and Joan Robinson's *Economic Philosophy* (New York, 1962), as well.

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marginalists assumed that what Veblen called the principle of "natural liberty" was no longer adequate to explain or justify entrepreneurial conduct and income. Alfred Marshall, the English economist with whom Walker and Clark (and, for that matter, Arthur Hadley) had most in common in their search for a theory of distribution, explained that "the tendency of careful economic study is to base the rights of private property not on any abstract principle, but on the observation that in the past they have been inseparable from solid economic progress." While the natural right of property went unquestioned, the classical political economy's "failure to realize a positive service of capital in production" was not an omission that could undermine social harmony.[53] But once civil society could no longer be defined in terms of individual proprietors who were equally subject to the anonymous laws of the market—once a certain concentration of wealth and the permanence of a propertyless "wages class" were acknowledged—the probability of agreement among the constituent elements of a civil society on the most basic rights and obligations (of property, for instance) could not be assumed. Arthur Hadley, an authority on railroads from Yale University, who became another leader of the marginalist vanguard, described the problem: "We thus have a separation of the community into more and more rigidly defined groups, different in industrial condition, distinct in ideals, and oftentimes antagonistic in their ambitions and sympathies. This separation of laborers and capitalists into distinct classes involves serious dangers to society as a whole . . . It involves a contradiction between our political theories and the facts of industrial life. A republican government is organized on the assumption that all men are free and equal. If the political power is thus equally distributed while the industrial power is in the hands of a few, it creates danger of class struggles and class legislation which menace both our political and our industrial order."[54]

The search for a theory of distribution that attributed to capitalists a positive service in production was, then, a search for the principles and sources of harmony, mutuality, or equality among individuals aggregated according to economic function or social class. It was an anguished search made necessary by the rise of the "labor problem," by the demise of equality among individuals, and by the concurrent emergence of a modern class society. In that sense, the marginalists in this country may be said to have faced the question of class struggle squarely. Because they did not try to avoid the issues raised by the "socialistic writers," they were successful in enunciating a new language of political obligation that stressed the active service and progressive functions of capital.[55]

At this practical level, marginalism was a realistic approach to the central problem of modern industrial society. As such, it constitutes a significant

[53] Alfred Marshall, *Principles of Economics*, 2 vols. (variorum edn., London, 1961), 1: 48; compare Walker's references to Hadley and Marshall in "Source of Business Profits," 272 n., 275 n., and Marshall's reply in the same volume of the *Quarterly Journal of Economics*, 477, and following. Veblen's analysis of the principle of natural liberty is found in *The Theory of Business Enterprise* (New York, 1904), chap. 8.

[54] Hadley, *Economics*, 371–72.

[55] Again, see Dorothy Ross, "Socialism and American Liberalism," and Furner, *Advocacy and Objectivity*.

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contribution to what we know as modern liberalism. Like corporatism, modern liberalism proposes equity of functional groups, not equality of individuals, as the central principle of political obligation in a society characterized by the concentration of wealth and income.[56] But it is difficult to account in those terms for the success of marginalism as a teaching device in the universities and a model relevant to policy-making in the industrial sector. We need to know what made it realistic in these spheres of enterprise. We need to ask, in other words, why it was not until the 1890s that marginalist analysis was finally recognized as "useful in solving practical economic problems."[57]

If we can judge from discussion among the leaders of the business community, the events of 1884–94 had defined the relation between costs, prices, and returns on capital investment as the critical and immediately practical economic problem. Their solution was to consolidate competing firms and to integrate separate industries. This strategy of "rationalization" would reduce fixed costs as a proportion of the value of finished products and, in the longer run, allow the conscious adjustment of supply to demand and price to cost. But if supply rather than price or profit was to be the new, managed variable in the new market-investment system, the technical and administrative problem of cost allocation among different kinds and different uses of capital assets acquired a practical significance it did not and could not have so long as the determination of prices and profits was left to the demand side. Marginalist analysis acquired special relevance in this context. The theory of opportunity cost, for example, by which one can calculate the utilities that could have been produced with the same resources that went into what actually was produced, is essentially a method of establishing priorities in economic planning, for it assumes that one can choose between at least two different uses of assets. The principle of substitution (capital for labor and *vice versa*), which underlies neoclassical capital theory, is similarly predicated on the assumption that investment decisions are not forced on capitalists by external circumstances, as Jenks suggested they were as late as 1890: "No sooner has the capitalist fairly adopted one improved machine, than it must be thrown away for a still later and better invention, which must be purchased at dear cost, if the manufacturer would not see himself eclipsed by his rival."[58]

Marginalism could not become useful in solving the practical economic problems of cost allocation and long-term investment planning until the corporate alternative to unrestricted competition among capitalists made them practical problems. By the same token, marginalism's broader view of the capitalist's active

[56] William A. Williams and Ellis Hawley are among the few historians who have explored this aspect of modern U.S. liberalism: see Williams, *The Contours of American History* (Chicago, 1961), Part III; Hawley, "Herbert Hoover, the Commerce Secretariat and the Vision of an Associative State," *Journal of American History*, 61 (1974–75): 116–40.

[57] Goodwin, "Marginalism Moves to the New World," in *Marginal Revolution in Economics*, 298–99.

[58] J. Jenks, "The Economic Outlook," 252. Compare Robbins, *Evolution of Modern Economic Theory*, 18, and Schumpeter, *History of Economic Analysis*, 900–17. See David I. Green, "Pain-Cost and Opportunity Cost," *Quarterly Journal of Economics*, 8 (1894): 218–29, for the original American formulation of a theory of cost that went beyond the framework of "abstinence" and "waiting" as explanation of entrepreneurial income.

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function in production could not be considered plausible until managers displaced skilled workers as the arbiters of the division of labor in the factory and elevated their mental labor to a position of dominance. From this standpoint, the marginalists carried the day in the last decade of the nineteenth century because they offered a model of modern industrial society that presupposed changing it so that capital's solidarity and discretion could realistically be depicted as the conditions of economic growth and social harmony. Marginalism was, in sum, both symptom and attempted cure of the late nineteenth-century's prolonged crisis of competitive capitalism. As Hollander suggested, "the successive aspects of the theory become intelligible as the interpretation, graphic, though not ultimate, of contemporary industrial life."[59] The marginalist revolutionaries of late nineteenth-century America understood that the point of their theory was to change industrial life, and they were able to help change it because their theory did in fact comprehend their times.

THESE PROPOSITIONS BRING THE SOCIAL ANALYSIS OF late nineteenth-century economic history full circle, for the rationale of that analysis is derived from the post-Keynesian critique of marginalist distribution theory. But if marginalism triumphed because it was realistic or became realistic under corporate conditions, what validity can the critique and the rationale be said to retain?

Political economy began as moral philosophy, a fact often cited as an irony of modern times. But it is not ironic, for a simple reason: the explanatory scope and value of moral philosophy and economic theory must ultimately be judged according to the same social criterion. Both species of argument seek systematically to explain the relation between intentions and actions; they assume that a relation specified in the argument can be embodied in the real world.[60] Economic theory is usually more pointed about its claim to validity in the real world, but the difference between economic theory and moral philosophy in this regard is one of degree, not kind. The world that marginalism would embody, if transposed from theory to practice, is a world in which the "psychological relation between individuals and finished goods" dominates or replaces social relations: in other words, it describes a world not very different from the one we inhabit. Hence, marginalism is not merely realistic; it is elemental reality. But that does not mean it is immune to criticism. Reality is not necessarily rational except in the sense that it is explicable in historical terms.

The analysis offered here does not purport to make the historical reality of the late nineteenth-century United States completely explicable. It probably raises more questions than it settles. For example, if the corporate system was indeed an economic solution to a social stalemate, how can we measure its success in social terms? What sort of historiographical agenda is outlined by provisional acceptance of a "social analysis" of late nineteenth-century economic history and theory? Will more dialogue between fields suffice? Or must we yet again confront the

[59] Hollander, "Residual Claimant Theory of Distribution," 278.
[60] See Alasdair MacIntyre, *After Virtue* (London, 1981), chaps. 3, 15.

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methodological issues raised by Marx, Veblen, and Schumpeter with respect to the character of economic change and innovation?

A new interpretation of late nineteenth-century U.S. history cannot, of course, be established by historiographical controversy, that is, without further research on the period. But we may be in a position to question the explanatory adequacy of the new economic history and the "organizational synthesis," and so to ask new questions about the nature of historical change in American society after 1870.

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Reviews of Books

GENERAL

NATHAN ROSENBERG and L. E. BIRDZELL, JR. *How the West Grew Rich: The Economic Transformation of the Industrial World.* New York: Basic Books. 1986. Pp. xii, 353. $19.95.

The epoch-making acceleration of Western economic development in the nineteenth century gave rise to famous studies that sought to explain the process from beginning to end. Recently, such sweeping investigations have almost disappeared, victims of ever-increasing academic specialization and the tendency of a generation of economic historians to discount the significance of developments before the Industrial Revolution. Thus, an ambitious interpretive synthesis by a distinguished economic historian and a legal expert that dares to examine the whole of Western economic growth deserves serious consideration.

Why, the authors ask, did the West develop economically, breaking the age-old cycle of povery and dramatically improving standards of living? Nathan Rosenberg and L. E. Birdzell, Jr. note that current explanations concentrate on science and technology. Yet, they contend this answer is partial at best. China and the Islamic nations once led the West in technical and scientific achievement, and the recent experience of the Third World shows that the teaching of science does not transfer the keys to economic growth. The authors search, therefore, for a more fundamental cause, and they find it in the emergence of exceptional economic institutions that account for the West's record of innovation and technological progress. The most important of these economic institutions, which had their origins in the late Middle Ages and early modern period, were the emergence of an autonomous economic sphere escaping from political and religious control and the development of a merchant class. These institutional arrangements regularized both economic and organizational change, whereby highly diversified private enterprise became the primary economic decision maker and carried forward decentralized experimentation that produced successful innovation. As the authors argue in successive chapters, this unprecedented system of decentralized, diversified private enterprise brought economic growth through trade in the late medieval and early modern period, through empirically derived inventions in the Industrial Revolution, and through harnessing scientific research for the development of products in the last hundred years. The vital constants have remained the autonomous economic sphere and highly diversified capitalist enterprise.

In assessing this interpretation, one is struck by similarities with some earlier sweeping explanations, notably those growing out of the nineteenth-century German school of historical economics. The same stress is placed on late medieval origins, the importance of institutions, and the development of capitalism. Yet much here is new and different. Above all, Rosenberg and Birdzell have crafted a path-breaking microeconomic history of Western economic development, a history that puts the universe of highly diversified business firms and their strategies at the center of the process. Gone is capitalism as an abstract, disembodied system, as well as previous fascination with governmental initiatives, capital accumulation, exploitation of labor, and colonialism and imperialism. Ultimately, the West's economic transformation depended on the formation of a dynamic, pluralistic society that gave all-important autonomy to scientists and intellectuals as well as to merchants and that reflected the lack of effective controls in a politically divided Europe.

The insights gained from the emphasis on the firm are fresh, stimulating, and powerful. It is fascinating to see most of Western economic history rewoven into a single fabric, rather than forever torn apart by the Industrial Revolution. Moreover, the authors' approach to firms in history is truly interdisciplinary. The usual economic analysis of business enterprise, whether individual or collective, is greatly enhanced by systematic consideration of firms as legal entities and as agents for experimentation and innovation. Rosenberg is a leading expert in the history of technology, and his analysis of the

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interaction of economic, scientific, and organizational factors in the development of Western industrial technology strikes me as highly original, even brilliant. Scholars from a variety of fields will undoubtedly profit from his stimulating insights. The authors also use suggestive excursions in comparative history, ranging from ancient China to the contemporary Third World, to test their interpretation and to strengthen their case. And they write well, using of necessity a specialized language but explaining terms and concepts with a clarity rarely found among American historical economists, who normally write only for one another. In short, the concentration on business enterprise has resulted in a consistent, provocative, and probably influential reassessment of the West's long record of economic growth and technological change.

Inevitably, such a wide-ranging study has limitations. Rich in ideas and analysis, it rests on stylized facts and rather casual historical research. The stimulating discussion of the Middle Ages seems nonetheless out of date and chronologically flawed to this nonspecialist. The treatment of the Industrial Revolution in England is rather superficial, and readers of David S. Landes will find few surprises, except, perhaps, an excessive optimism regarding the social consequences of industrialization. Continental industrialization is not considered, perhaps because the secondary literature customarily assigns a major role to governments, which is rather awkward for the authors' thesis. The discussion of the period after 1880 is devoted almost exclusively to the United States, now apparently synonymous with the West. One ends with the feeling that some of the best ideas are more nearly hypotheses for future research than well-buttressed conclusions. Finally, the authors sometimes overdo their probusiness bias. In their energetic defense of everything from early factory management to the creation of admittedly monopolistic trusts in the 1880s and today's mania for takeovers, they flirt with a polemical stance that undercuts confidence in the judiciousness of their basic interpretation.

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THOMAS J. J. ALTIZER. *History as Apocalypse.* (SUNY Series in Religion.) Albany: State University of New York Press. 1985. Pp. 265. Cloth $34.50, paper $10.95.

I have tried but failed to think of a kind thing to say about this book. The book does not lack ideas. Scarcely a sentence is not compounded of numerous totally incompatible ideas, closely linked. The book purports to be an essay on the whole meaning of "history," discovered through examination of meanings deeper than those previously noticed in "the epic tradition of Western literature." The jacket describes it as "a reenactment of the history of Western consciousness."

Thomas J. J. Altizer expresses his special indebtedness to Nietzsche and Joyce among modern spirits. People who have dealt with these thinkers will know that patience in dealing with paradox, allusion, metaphor, and so on brings rewards—although different people of equal goodwill value differently these rewards. Our author believes that having trafficked a great deal with these and other masters licenses him to write like this: "The epics of Blake and Joyce . . . embody a new cosmos which is chaos and cosmos at once, and such a world or cosmos must of necessity break down and reverse all given or manifest sources and forms of order. But an epic reversal and shattering of that order is just as fully a conceptual as it is an imaginative act, for it unthinks order itself, or all established order, and thereafter and therein an integral and unitary order and structure can never truly or actually be thought again" (p. 14). I submit that no number of patient rereadings is ever going to wring meaning out of such lines. There is paradox and there is paradox, and there is also blarney.

The author himself frequently forgets what he has said. I count at least six times before page eighty-one that "true self-consciousness appears" (or is "realized," or "manifested" or whatever) "for the first time in history!" Various and sundry other "revolutionary transformations" are going on at the same time, these sometimes located in the evolving epic tradition, at other times in art, in Hebrew prophecy, and so on. *Inter alia miracula* is the discovery of both the hand and the face: "By the time of *Apollo of Piombino* (*circa* 500 B.C.E.), Greek sculpture is almost fully born . . . It is not only an individual figure who is present here, but a world or cosmos of dawning light in which head and torso are one, and in which face is body and body is face. Thereby face knows a repose which had never previously been present in art, a repose anticipated by the Cycladic heads of the late third millenium. But those heads and their archaic counterparts are faceless, whereas the face of this Appollo [*sic*] is truly face, even if it is a face which is interior and exterior simultaneously, a face in which body and soul are one" (p. 20–21).

Lest readers feel professional obligation to seek out and read this book because the word "history" appears in its title, they should be forewarned that Altizer uses the words "history" and "historical" in so many irresponsible ways that they are completely emptied of content before he is done. His use of the word "apocalypse" (the other half of the title) is equally undisciplined. Other favorite words of the author that I used to believe that I understood include "primal," "advent," "resurrection," "scrip-

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ture" ("Epic poetry is everything that the great body of our literary critics and scholars assure us that poetry is not. First of all, it is Scripture" [p. 12]), "archaic," "sacral," and "consciousness." (Incidentally, the text bristles with printing errors, spelling errors, grammatical errors, and untold deviations from normal style, some of which may strike other readers as courageous.) When Altizer is briefly (fortuitously?) intelligible, he is preposterous. For example, we are told that "neither Islamic nor Jewish thinkers even attempted an integration of reason and revelation" (p. 103). Most infuriating in these instances is that one cannot assess the possible grain of truth because Altizer provides no scholarly apparatus at all. His explanation for this is, I believe, meant to be engaging: "Though each section of this book is the consequence of years of study, documentation is absent, both to avoid the appearance of a false authority, and to seek a postmodern style (scholarly documentation is historically confined to the modern age) . . . There are occasions herein when I have found the very words of a master scholar to be so overwhelming that I have simply paraphrased them rather than be willing to dilute their power" (pp. 3–4).

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F. S. NORTHEDGE. *The League of Nations: Its Life and Times, 1920–1946*. New York: Holmes and Meier. 1986. Pp. x, 342. $39.50.

For fifty years supporters and opponents have gnawed contentedly on the old bone. Why did the League fail? It wasn't tried, said the idealists. It couldn't work, said the realists. F. S. Northedge, his heart in the first camp and his head in the other, is the latest to struggle with this badly posed question.

Northedge the idealist opens with Marie Curie's vapidity on "a grandeur that commands our support." He nostalgically reviews the terms of the Covenant, that wonderous instrument forever waiting to be used. Alas, the record will not bear much sentiment, and Northedge the realist concludes that the League's failure to fulfill its main purpose, the collective maintenance of territorial security, was inevitable.

The problems arising from the conflicts of the 1930s, change against stability, and definitions of aggression could not be settled a priori nor in a forum. Against violent threats to the status quo no collective opposition, no opposition of any kind, gathered. The League's main members preferred appeasement, but they were no better off with secret diplomacy than they would have been had they embraced the rickety institutional shadow of the Covenant. Northedge's general conclusion, I infer, is that a successful system of collective security is (and was) impossible, because, in a world of change, the requisite support cannot be assured in advance.

In a disorderly world states will seek individual advantage and will not be tied to a rigid framework or definition. At least this is what Northedge sees in the 1930s, and he is not wrong. One only has to regard the antipathy between Britain and France to see how fragile was the prospect of cooperation at Geneva. So deep were differences between states, so fluid the international environment, that even universal membership would not have meant a League united around a Covenant, whose terms most distrusted and many hated, any more than an organizational open house has united states behind the Charter of the United Nations.

The historical core of the book is a conventional account of the League's institutional fortunes, from a British perspective, based on English-language sources. Nothing here is new, although to read about the fate of the disarmament and economic conferences and to contemplate the charade of mandates is always salutary.

Northedge's question, why did the League fail?, is a poor way to frame a historical discussion. It ties the argument to a wish, not to the fact. The measure of judgment is not clear. Are we to assess the League as a dream or as an institution? We must not impute to it an identity, a centrality, an initiative, even an adjunctive position, it did not have. The League was never the governor of a multistate system. It should not be judged on its own terms. It is likewise inappropriate to consider the League as an "experiment."

The League existed as ideal, as institution, as public myth, and as only one, even if many-featured, attribute of the political affairs of the time, and it should be studied historically in this broader perspective. In these terms, one need not even start from Geneva. For instance, governments often referred to the League in making decisions that had nothing to do with enhancing collective security or fulfilling obligations of membership but that were domestic in origin and purpose. Double policies and hypocrisy, motivated by the wish to stay in power, were the standard in Britain and France, as governments sought approval of the pro-League Left by expressions of support yet avoided any collective commitment in practice. The League was found to provide excellent cover for doing nothing or for (ill-fated) exercises of diplomacy of the old style. Hitler and Mussolini, conversely, mustered anti-League sentiment to their side in the name of aggrieved nationalism. States have rarely been more isolated than during this era of the society of nations.

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Perhaps also the history of the League needs a new periodization. The regional, Western orientation that Northedge describes was replicated in the United Nations. A repatterning might stake out 1920–60 as the period of League I and a renewed League II, with a war in between. After that, in the early 1960s, states of the Third World took over the organization from the remnant of the old League, which, by then had again lost interest in this form of collective security.

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MARTIN JAY. *Permanent Exiles: Essays on the Intellectual Migration from Germany to America*. New York: Columbia University Press. 1985. Pp. xxii, 328. $30.00.

This collection of fifteen masterly essays published during the past fifteen years acquires its unity from Martin Jay's continuing interest in the Frankfurt School of Max Horkheimer, Theodor Adorno, and their colleagues. As is typical of Jay's work, each essay or chapter is devoted to an individual thinker or a small group. The first half of the volume considers the ideas and projects of the Frankfurt school. Because this section examines material that Jay and others have analyzed in long monographs, I found it disappointing. "Adorno in America" is a lively piece, however, and some readers may find "The Frankfurt School in Exile" a useful introduction to the school and its relationship to Hegel, Marx, and Freud. The earliest essay, a critique of the utopian strain that sets Herbert Marcuse apart from the rest of the school, is still worth reading for its illumination of the roots of Marcuse's political ideas. Also noteworthy is an essay on the school's hostility to Karl Mannheim's sociology of knowledge.

The essays in the second section are devoted to other German emigrés of the 1930s and 1940s. Their relationship to Siegfried Kracauer, as well as to the Frankfurt school, provides a thread joining the essays in this section. Regrettably, Kracauer is known in the United States mainly for his erratic masterpiece on the German cinema *From Caligari to Hitler* (1947). Pointing to formative influences such as Max Scheler's phenomenology, Jay shows that Kracauer later elevated an ambivalence about publicity into a principled attempt to conceal his person and publicize his work. Here Jay is at his best, combining biographical digging, psychological acumen, and an analysis of Kracauer's work informed by a sound grasp of twentieth-century German philosophical tendencies.

The last substantial piece is "The Political Existentialism of Hannah Arendt." Jay effectively places her approach within the tradition of German existentialism, primarily Heidegger's philosophy, but this essay dramatically exhibits a weakness in the entire collection. In his introduction Jay invokes the authority of Roland Barthes to justify his refusal to make substantive alterations in the essays. Although we may be interested in the *Urfassung* of the works of Jay's subjects, his work belongs to the corpus of secondary literature subject to correction and updating. The perils of not revising essays for republication are brought home by the final sentence in his essay on Arendt, where he permits a badly crafted passage to stand: "Political existentialism was an obfuscating ideology in the 1920s, and one not entirely blameless in the rise of fascism; it must be judged no less of an ideology half a century later, although luckily its potential to blight our lives is far smaller than it was when Hannah Arendt, ironically fleeing from fascism, first succumbed to its dangerous charms" (p. 256). I accept Jay's statement that he was referring to the "charms" of political existentialism, not fascism (pp. xix–xx), but this misshapen sentence should have been rewritten to remove the ambiguity.

WALTER STRUVE
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DANIEL R. SMITH. *The Influence of the Fabian Colonial Bureau on the Independence Movement in Tanganyika*. (Monographs in International Studies, Africa series, number 44.) Athens: Ohio University Press. 1985. Pp. x, 89. $9.00.

The role of European interest groups in pressing their governments to grant independence to their colonial possessions after World War II has not been extensively treated in scholarly literature. This book begins to fill that void. Daniel R. Smith's brief, readable monograph is an analysis of the lobbying activities of the Fabian Colonial Bureau (FCB) in England (1945–61) on behalf of Tanganyikan independence. The author considers the activities of the FCB in influencing the position of the British Labour party on Tanganyika's future and its relations with African nationalists in the colony and with the United Nations Trusteeship Council.

Smith's thesis is that, throughout the period under study, the FCB consistently offered well-developed, rational, and balanced plans for the evolution of Tanganyika from a trusteeship of the United Nations to an independent state. The FCB is styled as having had decisive influence not only on parliamentary deliberations on Tanganyika's future but also on the platforms of the nationalists themselves. The Fabians are described as rational, calculating, humanistic, and yet more "radical" than Af-

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rican nationalists in their demands on the home government.

The strength of this monograph is in its insights into the role of one European pressure group in lobbying for African independence. Yet, this is also its weakness. The case is overstated at the expense of African nationalists. Reading between the lines, one gets the impression not of the FCB "orchestrating" Tanganyika's independence but of African nationalists, namely Julius Nyerere and the other leaders of the Tanganyikan African National Union (TANU), using leverage both at home and abroad (in the United Nations and England) to force independence. The author suggests that the Fabians developed TANU's program (p. 52). Nyerere did consult regularly with the FCB and was grateful for its advice and support. But this influence need not be construed as decisive. In fact, the author notes without much discussion that between 1958 and 1961 TANU began to chart its own course. The FCB stayed by the nationalists' side but had little influence in determining the party's platform at the critical stage of the independence movement. The Fabians may have had more influence in the British Parliament, but this is not clear.

The book's focus, brevity, and methodology limit its utility for the student of African history and politics. The author never explains the attraction of Nyerere and other nationalists to the Fabians. The Fabian philosophy is not systematically discussed. Instead, the FCB is viewed as "messiah." In fact, African nationalists had a natural affinity for the Fabians' egalitarian, democratic ideology; it suited their own purposes.

Because the author concentrates so narrowly on the FCB, he fails to identify other possible influences on the decision to grant Tanganyika independence or to consider larger issues of empire. Related to this is his failure to go beyond an analysis of public documents. No doubt living participants in these events could have provided valuable insights that are missed in the public record. For instance, what do Oscar Kambona or Nyerere have to say about their motives in dealing with the FCB? Were they "naive" clients of moralistic paternalists? Or were they rational actors who cultivated allies in pursuit of an end that they determined? These questions are still to be answered.

EDMOND J. KELLER
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BRITON COOPER BUSCH. *The War Against the Seals: A History of the North American Seal Fishery*. Kingston, Ontario: McGill-Queen's University Press. 1985. Pp. xviii, 374.

This is the story of a killing. In the past one hundred fifty years approximately fifty million seals have fallen victim to gaff and gun. This book by Briton Cooper Busch documents, as no other study previously, the quest for profits and the ensuing slaughters by North American sealers who ranged the world's oceans and seas. To be fair, Busch has not set out to write in either an antisealing or prosealing vein; although he leans toward an antisealing stance, his study displays diligent scholarship, balance, and a relative impartiality about a subject that has aroused so much contemporary passion. Nevertheless, conservationists and preservationists will find much grist to commend this work to their cause.

The aim of the book is not to document the brutality of the hunt, although the inclusion of graphic and bloody reminiscences of earlier times makes Newfoundland sealing seem pale in comparison, but to understand and analyze the growth and development of the industry and its impact on places and people. The vitality of New London, Connecticut, St. John's, Newfoundland, and Victoria, British Columbia rested on the seal hunt; for better or worse, the fortunes and lives of Newfoundlanders scattered in remote outposts and the Aleuts of the isolated Pribilof Islands—a sad tale in itself of escalating dependency—hinged on the animal and the whims of distant markets.

In describing this interaction between man and animal—admittedly, a one-sided affair—the author has gone to admirable lengths to portray the totality of the relationship in a skillful blending of the scientific and nonscientific. The biological uniqueness and particular frailties of diverse species and herds—otter, harbor seal, sea lion, harp seal, elephant seal, and the like—are outlined, as is their value to nineteenth- and early twentieth-century man. Fascinating, too, are the diplomatic controversies stretching from the curbing of pelagic sealing in the late nineteenth century to the present-day disputes involving Newfoundland hunters of the "cute" harp seal and the antifur lobby so successful in Western Europe. In Busch's work we have an excellent and readable history of North American seal fishery but not to date of that of other countries that have "gone a sealing."

RICHARD J. DIUBALDO
Concordia University

ANCIENT

ARTHER FERRILL. *The Origins of War: From the Stone Age to Alexander the Great*. New York: Thames and Hudson; distributed by W. W. Norton, New York. 1985. Pp. 240. $19.95.

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The attempt to write on this broad subject, across time and diverse civilizations, may seen daunting, but Arther Ferrill executes the task superbly. Inevitably the enterprise is a work of synthesis, but this book is by no means merely that; Ferrill persuasively advances an original thesis, and he presents numerous interesting subordinate ideas as well. His central themes are that warfare is at least as old as human history ("as soon as man learned to write, he had wars to write about" [p. 31]) and that the major developments of ancient warfare—in weaponry, military organization, tactics, and strategy—arose in the ancient Near East and later influenced classical Greece. These developments occurred along parallel lines, one starting in the ancient Near East, the other in Greece. The contributions of each came together and were effectively blended by Philip and Alexander of Macedon. Ferrill's object is to describe, as far as possible, conditions of combat on land and sea rather than to discuss the causes of individual wars or their social, political, or economic effects. Although Ferrill treats military history, especially technological innovations in warfare, at length, his book is not a narrow, technical work. Studded throughout with comparisons to later times and analogies to other military figures and battles, the text engages the reader's interest and moves crisply and clearly through a staggering amount of material.

The first half deals with warfare in the ancient Near East, from the prehistoric to the Persian period. Here Ferrill uses rather scanty archaeological evidence to discuss the rise of organized warfare (defined as "formation") and developments in weapons and tactics. The theoretical discussions of these issues, skillfully interwoven with treatment of anthropological and archaeological data, provide a framework for later chapters. Ferrill next discusses military innovations dependent on the discovery of the use of metals; the tactics and strategies of the great Bronze Age empires, the Egyptian and Hittite, which clashed in combat at Kadesh in Syria; and aspects of Babylonian and Biblical warfare, including the use of chariots, fortifications, and siege warfare. The third chapter treats the great Assyrian and Persian empires, with special reference to their grand strategies and tactics (such as the Assyrian use of gruesome pictorial propaganda as an early form of psychological warfare). The emphasis on cavalry, siege equipment, and the rise of the navy in this period is well brought out.

A trained classical historian, Ferrill is at his best in the second half of the book, devoted to warfare in Greece. The emphasis in the chapter on classical Greece is on the superb hoplite armies of that era as well as on the growing importance of fleets, especially in the Athenian empire. The major battles of the Persian and Peloponnesian Wars are analyzed intelligently and clearly. But, in the next chapter on the military revolution (399–340 B.C.), Ferrill begins to weave together the threads of his story by demonstrating that knowledge of Persian military practices was gradually spread to Greece through the experiences of Xenophon and the Ten Thousand and mercenary commanders such as Iphicrates. The increased employment of light-armed infantry (peltasts), specialty troops such as archers and slingers, and of cavalry and the invention of catapults with attendant changes in siege warfare are traced to Asian origins. Philip forged these elements into an integrated army, with which he conquered Greece. Alexander inherited this army, but he used it brilliantly in his conquest of the Persian empire. In the final chapter Ferrill analyzes Alexander's major battles and cogently argues not only that Alexander was an outstanding tactician and general but also that he perfected the art of war in antiquity so that few modifications, even those such as the stirrup and gunpowder, had any substantial impact before the twentieth century. Ferrill is no mere armchair general in this section, for he writes clearly and persuasively and criticizes the views of several modern military historians who have found fault with Alexander. Ferrill argues for the veracity of ancient accounts, especially Arrian's, of Alexander's battles. Here again numerous useful and instructive comparisons to later situations are made, culminating in an interesting section entitled "Alexander at Waterloo." This solid and readable book should interest the historian of antiquity, the military historian, and the general reader alike.

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I. S. MOXON *et al.*, editors. *Past Perspectives: Studies in Greek and Roman Historical Writing.* (Papers Presented at a Conference in Leeds, 1983.) New York: Cambridge University Press. 1986. Pp. ix, 241. $39.50.

This book consists of the papers delivered at a conference convened by I. S. Moxon and his coeditors on Greek and Roman historical writing. The editors contribute a brief prologue, an epilogue of nine pages, a bibliography listing the works referred to in the papers, a general index, and *index locorum.* The volume contains ten papers, ranging chronologically from Herodotus to Ammianus Marcellinus and representing a variety of approaches to ancient historical writing. The editors do not explicitly state that the papers were revised for publication, but some cross references are given. The contributors appear to have been accorded a wide freedom in selecting aspects of historical writing for investigation.

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The epilogue concludes that "historians who wish to understand the ancient world should show themselves alert to the nature of the literary sources on which their evidence depends" (p. 211). In practice, the attitudes of the contributors to the sources are quite diverse. In part this depends on the quality of the authors being investigated, but the contributors themselves divide into believers and skeptics in their approach to the sources: some appear to regard the ancient writers as engaged on virtually the same task as modern historians of the ancient world; others concentrate on the elements of literary stereotype and sheer invention exhibited by the sources. For example, in an otherwise exemplary survey of military engagements in Xenophon's *Hellenica*, in which he argues for the superiority of Xenophon's accounts, C. J. Tuplin takes it for granted that Xenophon had at his disposal the means for accurate reconstructions of these battles. Tuplin does not consider whether the nature of ancient battles allowed even the participants the possibility of reconstructing them, and he deliberately sidesteps the question of what, if Xenophon's accounts are superior, the conflicting tradition has as its aim. In contrast, in a discussion of the treatment of barbarians by Ammianus Marcellinus, T. E. J. Wiedemann demonstrates the historian's debt to literary convention and concludes that Ammianus did not even intend his statements about barbarians to be regarded as descriptive ethnology. Wiedemann contends that Ammianus's audience expected that in describing barbarians the historian would avail himself of the literary stereotypes about such peoples (which can indeed be traced back to Herodotus). But, if so, expectations in this area are only one element of the audience's influence on an ancient writer. As Clemence Schultze argues from the case of Dionysius of Halicarnassus, an ancient writer's selection of themes was likely to be influenced by his perception of the concerns of those for whom he was writing. A bias is thus built into the acount over and above the bias imparted by the writer's own interests and manner of thought. The writer's bias is admirably demonstrated in the instance of Plutarch's Roman *Lives* by C. B. R. Pelling, who shows how Plutarch's essentially Greek view of Roman politics distorts by its emphasis on conflict between *demos* and *boule*. A similar, if milder, emphasis is detectable in Dionysius of Halicarnassus and, to a more limited extent, in Appian and Dio. It is indeed a matter of some concern that so much of our knowledge of Roman history is transmitted by Greek authors and therefore all the more to be welcomed that, under the influence of Emilio Gabba and others, they are now beginning to receive attention on the scale they deserve.

The history of early Rome and the middle Republic acutely raises the problem of the nature of our literary sources. Two contrasting solutions are offered here: T. J. Cornell believes that Roman tradition on these early years is essentially sound and points out that the skeptics' case is based on reconstructions of fragmentary historians. But his own case is no stronger; "it may be," he writes, "that the pontifical chronicle contained a systematic record of annual events" (p. 82). However that may be, one would wish to see the authority of the tradition defended on the basis of a larger number of specific incidents. T. P. Wiseman on the other hand, in "Monuments and the Roman Annalists," skillfully suggests that later historians could invent incidents, either by misinterpreting a monument or exploiting the disappearance of one. For him "the proximity of history-writing to fiction and the novel" is an important subject "now at last receiving proper attention" (p. 88). The claim is promptly countered in the adjoining paper by Elizabeth Rawson, who dismisses "the fashionable notion that ancient historiography is a branch of ancient fiction" (p. 118). Her useful paper, however, by investigating the differing attitudes to Cassius and Brutus adopted by later historians, shows how variable a historical tradition may become, even if the writers broadly reflect late republican views. In this case, as in T. J. Luce's study of "inconsistency" in Tacitus, and indeed in the collection as a whole, more might have been said about the role of rhetoric (which all writers had studied) in the composition of historical narrative.

In addition to the papers mentioned above, J. Cobet writes on the causes of war in Herodotus and Thucydides, and J. D. Smart examines Thucydides' choice of chronological scheme.

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GIUSEPPE ROCCATAGLIATA. *A History of Ancient Psychiatry*. (Contributions in Medical Studies, number 16.) Westport, Conn.: Greenwood. 1986. Pp. viii, 296. $45.00.

Ludwig Edelstein's "misapplications of historical analogies" aptly describes this well-intentioned but dreadfully misleading book. By attaching the label "psychiatry" to the approaches of medicine by all cultures from Cro-Magnon times to the later Roman empire, Giuseppe Roccatagliata forcibly warps ancient thinking to fit modern preconceptions, "handing out medals to moderns" to borrow the phrase of John Riddle. At least one is not pummeled by late twentieth-century psychobabble in the details of the "psychotherapy" of Homer, Hippocrates, Celsus, Soranus, Galen, and other great figures of ancient medicine. This lack of modern jargon may owe more to Roccatagliata's use of Renaissance and Enlightenment texts (always in Latin) in an oddly

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misjudged assumption that such printed editions would preserve a better sense of the ancient concepts, even when the original might have been in Greek. As any student of Renaissance medicine well knows, once the Greek and Roman medical classics began appearing in print, editors, physicians, and classical scholars frenetically reedited those ancient texts, "updating" them to serve the current needs of medical practitioners; the Renaissance modernizing of Galen was especially marked, nicely illustrated by the Latin "translations" that accompany the still-standard Greek texts in *Cl. Galeni Opera omnia* (1821–33), edited by C. G. Kühn. The Latin renditions sometimes have little to do with the Greek texts; since the Latin translations in the Kühn edition are simply reprinted from Renaissance references, they are often openly rendered in terms of the "new medicine" of the fifteenth and sixteenth centuries. Not surprisingly, Roccatagliata found what he sought in these "ancient" texts; sadly, his hopeful hypothesis of the transition from ancient to modern psychiatry collapses in the face of the real texts of Hippocrates, Galen, and the rest, which have been carefully edited into a form as close to the original as possible by philologists since the nineteenth century.

Perhaps the most serious flaw in this book is the documentation of sources. When one looks up the apparently detailed and precise references to a rational rejection of magic by Pliny, Plutarch, Galen, or the hundreds of other classical sources that festoon the footnotes, one is continually frustrated by an inability to check those sources in any of the standard texts; the numbering system employed for the Latin texts is so at variance with any known international standard that one cannot verify Latin or Greek quotations in the Teubners, Loebs, or Budé series or in any other widely employed collection of classical texts published in the nineteenth or twentieth centuries. Moreover, many of Roccatagliata's quotations are peppered with ellipses, showing a continual omission of evidence that might conflict with the author's predilections.

If one is to treat ancient psychiatry (however one labels it), one must also be well acquainted with the continual presence of magic, dream lore, and astrology in all levels of ancient society. In *History of Magic and Experimental Science* (1923) Lynn Thorndike documented that magic eventually led to science and that purported conflict between ancient science and magic was often unsupported by the primary sources. By ignoring Galen's espousal of astrology in the service of diagnostics, Roccatagliata has missed a superb opportunity to show that ancient "psychiatry" was as subject to determinism as is the modern analogue. And, should one wish to sample the magical texts themselves, one can now gain access to most of them (Greek, Coptic, and Demotic) in the first volume of *The Greek Magical Papyri in Translation* (1986), edited by Hans Dieter Betz. By disregarding the Greek and Roman respect for magic and assuming that only scientists such as the Hippocratic physicians would reflect ancient notions of mental states, Roccatagliata has simply perpetuated a view of ancient science and medicine considered quite outdated for the last fifteen years. To gain a better picture of Greek and Roman "psychiatry," the interested reader should peruse Owsei Temkin's *The Falling Sickness*, 2nd ed. (1971), Bennett Simon's *Mind and Madness in Ancient Greece* (1978), and G. E. R. Lloyd's *Science, Folklore, and Ideology* (1983). The ancients certainly had their notions of mental illness, but the modernism of "psychiatry" brutally overdefines these concepts.

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THOMAS R. MARTIN. *Sovereignty and Coinage in Classical Greece*. Princeton: Princeton University Press. 1985. Pp. xiv, 331. $32.00.

Determining the chronology of ancient Greek coinages is often the most difficult problem confronting numismatists. Greek coins are only rarely dated and almost never related in any obvious way to historical events as, for example, Roman issues often are. To overcome this problem, scholars have traditionally employed a combination of historical and art historical analysis as well as (increasingly in the twentieth century) numismatic techniques (such as die-study and hoard analysis). Among the most basic and persistent tenets underlying the current dating of city-state coinages is that sovereignty and coinage were indivisible. Thomas R. Martin challenges this widely held belief on the basis of a meticulous study of Macedonian domination of Thessaly in relation to Thessalian coinage.

Some readers may find the title of the book somewhat misleading since more than half (over one hundred fifty pages) is devoted to the history of Thessaly. Martin's detailed treatment of the historical evidence provides, however, a solid (and for the most part convincing) framework for his more summary handling of the coinages themselves. The book is not a technical numismatic study: it does not, for example, contain any catalogue (or even list) of the fourth-century coinages minted by the Thessalians or any other of the now-usual elements of a modern numismatic study (for example a die-study, a comprehensive list and discussion of hoard evidence, and so on). In fact, there are only two plates (illustrating just nine coins), chosen not, it would seem, because of their indispensable importance to the argument of the book but because they happen to be in Harvard's Dewing Collection. Nevertheless,

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Martin skillfully integrates the results of his technical numismatic research into the historical discussion both of Thessaly specifically and of other Greek city-states (especially Ambracia, Sicyon, and the states of the fifth-century Athenian empire) whose numismatic history has been similarly reconstructed on what are shown to be unsupported assumptions.

The book is sure to generate controversy, and deservedly so, for the common belief that Greek coinages served as symbols of sovereignty akin to the flags of modern nations has become, as Martin points out in an excellent chapter devoted to the history of scholarly opinion, deeply but wrongly entrenched in the thinking of numismatists and historians alike. The time for reexamination is long overdue, and Martin's forceful presentation of a purely economic explanation of Greek coinages should serve as a model and starting point for future investigation. As he concludes, "if coinage really was an important affirmation of political sovereignty in the classical period, rather than an essentially practical response to concrete needs of the state, it would be difficult to understand why Greek states often took so long after the invention of coinage and the spread of its use to begin to produce their own coinage, and why they often went for long periods without producing any coinage once minting had been initiated" (p. 231). This is indeed a critical issue and a vexed question in Greek numismatics, and Martin's book clearly makes a major contribution toward the study of Greek coinages in their historical context.

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Emory University

ALEXANDER FUKS. *Social Conflict in Ancient Greece*. Jerusalem: Magnes, for the Hebrew University; distributed by E. J. Brill, Leiden. 1984. Pp. 363. f. 112.

The title of this book is more than slightly misleading, for it suggests a monograph on social conflict in Greek society rather than what the work actually is—a bare collection of articles by Alexander Fuks. The twenty-four articles that make up the book are connected by no editorial comment but are simply reprinted with a photocopying process that makes this volume the barest step beyond a bound collection of xeroxed materials. Further, a half dozen or so of the articles and notes, such as the two on the Jewish revolt of A.D. 115–17, have absolutely nothing to do with social conflict in Greece, and many of the others are at best on the periphery of the topic.

The majority of the articles, it is true, relate in one way or another to the question of social conflict, and the most generally informative is the first, "Patterns and Types of Social-Economic Revolution in Greece from the Fourth to the Second Century B.C." But this is followed by "Social Revolution in Greece in the Hellenistic Age," which essentially repeats part of the previous article and adds nothing new. Five excellent studies appear on Plato, Isocrates, and Aristotle on the question of social and economic conflict, and the remainder of those articles relevant to the topic deal with specific issues or examples of social conflict, such as the the redistribution of land and houses in Syracuse in 356 B.C. and its ideological aspects.

Little more can be said about this book, except to ask why it was published. The volume is certainly convenient for fans of Fuks or someone working on social and economic conflict in Greece, but the articles are readily available in their original form, virtually all of them published in mainstream classical journals. Is the convenience of a handful of scholars really sufficient justification for publication, especially in an age of economically strapped academic presses? Is there any good reason here for littering the shelves of university libraries with yet another repetitive scholarly tome?

Part of the aim of the editors in creating this book was to produce an "appropriate memorial to a beloved scholar and friend," but, by publishing a volume that is redundant and poorly assembled, they have fallen considerably short of this goal. Perhaps finances prevented them from doing anything more than reproducing the articles without editorial connection and comment, but one might then ask why the book was published at all. At the very least the product ought to have been more accurately labeled.

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University of New Mexico

DAVID WHITEHEAD. *The Demes of Attica, 508/7–ca. 250 B.C.: A Political and Social Study*. Princeton: Princeton University Press. 1986. Pp. xxvi, 485. $62.00.

The subject of local government in Attica is of fundamental importance in understanding the underpinnings of Athenian democracy. The standard work on the subject so far has been B. Haussoulier's *La vie municipale en Attique* (1884). The intervening century has witnessed not only the discovery of Aristotle's *Constitution of Athens* (1891) but also a plethora of archaeological and epigraphical discoveries, scattered in many different publications and appearing at irregular intervals. Further, the development of new analytical tools and the growth of the social sciences have made much of Haussoulier's treatment of the subject inadequate or obsolete. The need for a comprehensive collection and interpretation of all the material has been evident for several

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decades. It is good news indeed that it has now been filled, and admirably so, by David Whitehead's book.

No aspect of the deme is left unconsidered. The first section deals competently and comprehensively with the problem of sources. The second part, entitled "Inside the Deme," begins with a sociological survey of the inhabitants, political structure, and functions of the deme and proceeds to a discussion of its finances, its religious life, and its social values. The third part relates the microcosm of the deme to the macrocosm of the polis, especially in the election of councilors and magistrates and in the impact of local politics on the politics of the city-state. The book closes with six appendixes on matters of detail, including lists of all the 139 known demes, of the evidence for them, and of a prosopography of known deme functionaries.

Thorough mastery of both ancient sources and modern scholarly discussions make this book indispensable for the historian of ancient Greece. But it has also a lot to say to sociologists and to students of local government of any period. It is clearly and well written so as to be accessible to anyone with even a nodding acquaintance of Athenian government. Whitehead's book is as definitive a discussion of a central problem for our generation as any scholarly publication can be.

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W. KENDRICK PRITCHETT. *The Greek State at War*. Part 4. Berkeley and Los Angeles: University of California Press. 1985. Pp. ix, 278.

In this fourth volume of his continuing study of Greek warfare W. Kendrick Pritchett offers two monographs in chapter form. The first is entitled "The Pitched Battle." Pritchett collects the evidence for battles in Homer and archaic Greece and concludes that the "hoplite reform" of the seventh century is a myth, since the Greeks had been fighting pitched battles in close formation since Homeric times. The hoplite panoply was indeed perfected in the seventh century, but this acme of a long evolution did not result in major social or political changes. Pritchett's conclusions are controversial, but he has mustered very impressive evidence. Pritchett then turns to the historians of the classical and Hellenistic periods to determine the standard forms of hoplite battle. He concludes that some battles were won at the initial charge, when the enemy phalanx broke under the impact or when panicky hoplites abandoned the line. Battles not decided in the first shock were protracted, entailing hand-to-hand combat with spear, sword, and shield. Pritchett's rejection of recent arguments for individual combats within the context of phalanx battle seems decisive, and his descriptions of the varieties of battle are persuasive.

The second chapter concerns the burial of Greek war dead. Here Pritchett offers fewer general conclusions but amasses a vast quantity of literary, epigraphic, archaeological, and topographical material. He emphasizes throughout the importance of proper burial to the Greeks and the significance of requesting one's dead: he who requests the return of dead comrades concedes defeat by acknowledging enemy control of the field. Pritchett demonstrates that the Spartans typically buried their dead on the field of battle or in nearby friendly territory; the Athenians cremated their dead in the field and, beginning in circa 465 B.C., brought the ashes home for burial in the state cemetery.

Both studies are exhaustive; both show Pritchett's remarkable ability to deal with disparate categories of evidence. Few modern scholars would be so at home in combining, for example, textual criticism with topographical fieldwork. Pritchett is familiar with relevant sources of comparison, especially medieval and Near Eastern, and even ranges to the American army in Vietnam. His bibliographic documentation is impressive. No one interested in Greek warfare can afford to ignore this volume. Although much of the discussion is technical, Pritchett's translations of most Greek and Latin passages will make his work accessible to nonclassicists.

I have two quibbles: Pritchett's concern with defending the general accuracy of ancient authorities may occasionally lead him astray; for example, he accepts Pausanias's testimony on hoplites in the First Messenian War (p. 41). The vitriolic comments on page 94, note 1 are irrelevant and unworthy of a scholar of Pritchett's stature. In the same note he advises the editors of another's articles to demand exact quotations; the California editors might be urged to use a firmer hand with notes such as this one, which may raise questions in the reader's mind about the objectivity of the rest of the book.

JOSIAH OBER
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ANDRÉ GEROLYMATOS. *Espionage and Treason: A Study of the Proxenia in Political and Military Intelligence Gathering in Classical Greece*. Amsterdam: J. C. Gieben. 1986. Pp. 140.

Xenia, generally translated "guest-friendship," was an institution already known in Homer's time, out of which developed the *proxenia*. Normally (the case of Sparta is slightly more complex) a citizen of one Greek state would be designated by another state as

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its *proxenos*, that is, an official host, patron, and helper of its citizens in their dealings with his own city. *Proxenoi* of powerful states, such as Athens or Sparta, might come to identify themselves as strongly with those states—or their ruling factions—as with their own fellow citizens. André Gerolymatos effectively traces some of the implications of such divided loyalties. His limited topic is more accurately indicated by his subtitle than by the deceptively broad main title.

The reader cannot avoid feeling that a long article has here been inflated into a short book. Much of part 4 ("The Analysis") is largely repetition of points made in parts 1 ("The Problem") and 3 ("The Evidence"), and the lists of *proxenoi* in appendixes B and D repeat some of the same material yet again. Inflation is also evident in several needless discussions of distinctions within modern intelligence terminology. While inflating, Gerolymatos might as well have also added English translations of the many Greek passages, which would have made parts of the book more accessible to nonspecialists. That part 4 should include both conclusions and appendixes seems illogical, and appendix E ("The Role of the Proxenia in Inter-State Relations") should have been incorporated in the main text, probably in part 1. These organizational flaws, however, detract little from the book's usefulness, and sometimes they seem almost beneficial: its very repetitiousness makes this an easy book in which to find one's way around. The documentation is clear and ample, and anyone interested in the topic would do well to begin with this study.

A short review should not dwell on the several barbarisms and careless phrases or the numerous typographical errors. Readers should, however, at least be aware that every reference in the book to the islands of Keos and Kos is reversed, that is, that whenever Keos is mentioned, Kos is meant, and vice-versa. Items discussed, yet irrelevant, given the limits implied by the subtitle, include the assassination of Ephialtes (p. 17, no suggestion of any foreign connection), Alexander's spies (pp. 21–22, postclassical, and not *proxenoi*), and a third-century Boeotian *proxenos* (p. 79, postclassical, and no "intelligence" connection). Why is Xenias of Elis, in the Peloponnesos (pp. 34–39), discussed in a section devoted to northern Greece? Why entitle a section "The Ionian Islands" (p. 64), when in fact it refers only to the single island of Korkyra (Corfu)? No evidence suggests that Sestos (pp. 46–48) was either a member of the Second Athenian Confederacy or a rebel at the time of the Social War. Many questionable inferences mar the discussion (pp. 58–61) of events in Kolophon and Notion—for example, the sending of Athenian colonists, Apollonophanes' status as *proxenos*, and the description of his duties. Gerolymatos does not mention that all the inscribed provisions cited come from an amendment to an Athenian decree; we do not know what the subject of the main decree may have been. The inference (p. 63) that Nikias of Gortyn was "a non-Athenian in command of Athenian forces" is baseless; the discussion of textual passages in appendix C (p. 109) does not define the problem, that is, whether the first two Greek passages quoted refer to Nikias or to another (unnamed) person. It is highly questionable that "Demosthenes' advocacy of a hostile attitude toward Makedonia was consistent with Boeotian policy" (p. 77). Quite the contrary, Demosthenes was able only belatedly (in 339) to win the Boeotians, whose *proxenos* he was, away from their longtime friendliness with Philip II and into alliance with their traditional rivals, the Athenians. Moreover, his success led (after Chaironeia) to disaster for the Thebans, much more so than for the Athenians.

JACK CARGILL
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JOSIAH OBER. *Fortress Attica: Defense of the Athenian Land Frontier, 404–322 B.C.* (Mnemosyne, Bibliotheca Classica Batava, Supplementum Octagesimum Quartum.) Leiden: E. J. Brill. 1985. Pp. x, 243. $28.00.

There is much to praise about this book, and little to criticize. Josiah Ober sets forth a clear thesis, which, if not entirely novel, has never before been so thoroughly elaborated and sustained. Ober asserts that a series of changes in the political, economic, and military situation of Athens after its defeat by Sparta in the Peloponnesian War resulted in a new, defensive mentality; this mentality, in turn, produced an elaborate border fortification system intended to protect the Attic territory from invasion and ravaging, either by small bands of freebooters or by large enemy armies. The book is well organized, firmly grounded in solid and judicious presentation of the ancient evidence, both literary and archaeological, and persuasive in its conclusions.

Part 1 discusses elements crucial to an appreciation of fourth-century Athens. First, Ober examines the economic situation after 404 and demonstrates that, with the loss of the fifth-century empire and its revenues, Athens necessarily relied much more on the economic resources of Attica, based largely on agriculture, mining, and quarrying. These activities, which sustained more than half of the population, were particularly susceptible to interruption through invasion so that rural inhabitants favored a defensive strategy to protect their properties and livelihoods. Examining changes in warfare in the early fourth century, the next chapter demonstrates that the agonal system, in which a hoplite army invaded and threatened the harvest of the enemy to

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force a response from the opposing hoplite army, gave way to new tactics and strategies in which light armed forces, peltasts in particular, and mercenary corps revolutionized warfare. In fourth-century conditions more effective techniques of ravaging and pillaging brought new economic pressures to bear, and Greek states had no choice but to devise new strategies to combat them. Next, the rise of a defensive mentality in Athens is traced largely through the examination of rhetorical evidence. Ober argues that a majority in fourth-century Athens favored policies aimed at defending their territory against invasion, in marked contrast to Pericles' abandonment of the countryside to seek protection behind the walls of Athens.

Part 2 discusses various methods of defense from a theoretical standpoint and argues that a border defense strategy was preferred in Athens. Then, Ober examines numerous changes in the Athenian military establishment, including the training and employment of the *ephebes* as light-armed, border garrison troops; the establishment of a *strategos* specifically charged with the defense of the *chora*; and a new system of calling up reserves by age classes to meet emergencies. Ober concludes that fourth-century Athens had developed and implemented a border defense system, incorporating many changes in response to the new realities of the age.

What was this system, and how did it operate? Parts 3 and 4, the second half of the book, answer these questions. The routes into Attica, by which an invading army might travel, and the archaeological evidence for a system of forts, smaller fortified spots, and watch towers are examined in detail and illustrated by many useful maps. This catalogue is extremely valuable, and Ober makes a significant contribution to scholarship here. His presentations are based on published reports and, with few exceptions, autopsy. He describes routes and fortifications in detail and makes an impartial attempt at dating based on pottery and styles of military architecture. The system, he argues convincingly, was intended to allow border troops stationed in forts at the major passes into Attica or at the signal towers to alert the city when invasion threatened. These troops would attempt to hold off the invaders at the pass or, if that failed, to harass them from their forts until a military force from Athens could reach the scene. Historical considerations and archaeological evidence suggest that the system was put into place, with major construction of roads and forts, between 385 and 340. Although Ober perhaps relies too much on the orators, a notoriously tricky body of evidence, and tends to play down the importance of imperialism as a motive in Athenian foreign policy after 404, his principal thesis is not damaged thereby. His book is a major contribution to military history and to our understanding of fourth-century Greece.

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STEVEN W. HIRSCH. *The Friendship of the Barbarians: Xenophon and the Persian Empire*. Hanover, N.H.: University Press of New England, for Tufts University, Medford, Mass. 1985. Pp. xi, 216. $25.00.

Xenophon's experiences in the Persian empire and his ability to shape them into considered prose make him, as Steven W. Hirsch realizes, an interesting figure for the study of the interaction between Greece and Persia. Hirsch's monograph tries to understand how much this Greek reliably knew about the Persians and how acutely he appreciated them. It aims finally at permitting a more nuanced awareness of this relationship, which the influence of Isocratean panhellenism has too often simplified.

Hirsch commendably avoids many of the prejudices, convoluted arguments, and methodological circles bedevilling so much Xenophontic scholarship. He attests to Xenophon's extensive knowledge of the Persian bureaucracy, which is evident, for example, in his innovative use of Greek technical terms cognate with Persian originals. Using insights from Iranologists, Hirsch occasionally corroborates Xenophon's evidence or enriches a reading of a work with Persian material. Aware of the demands of genre and the importance of context, he proposes thoughtful approaches to the *Agesilaos* and the *Anabasis* and concludes that neither characterizes Xenophon as a thorough Persian-hater.

Yet Hirsch's zeal to establish Xenophon's reliability and discernment carries him at times both too far and not far enough. The key chapter on the *Kyroupaideia* seems intent on rescuing Xenophon from the charge of bald invention, as if invention were a fault in an epic novel. Stressing the work's historical or genuine folkloric details is rather like reading Homer for the boar's tusk helmets and Nestor's cups at the expense of an idealizing imagination. Plato surely did not object to the *Kyroupaideia* as a *plasma* or fabrication because he expected it to be factual and was disappointed. He had a serious quarrel with it as a literary mimesis in possession of the dangerous power to persuade with its charm. In a lengthy excursus on the institution of the "King's Eye," Hirsch, inspired by *Kyroupaideia* 8.2.10–12, shows that little satisfactory evidence exists for an organized intelligence service or even a top intelligence officer in the Persian empire. But he may go too far in claiming that the Eyes did not exist at all and that Xenophon here consciously established himself as an expert on Persian affairs against other earlier and contemporary Greek sources. Xeno-

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phon's use of the phrase "so-called" Eyes, which Hirsch cites to show that Xenophon did not believe in their existence, was simply a standard way of qualifying an unusual expression in Greek, and Xenophon's saying that the Eyes "were believed" to be many does not necessarily mean that the phenomenon was a hoax but that the number of such individuals was inherently uncertain since the king was receptive to all informants. Xenophon described a network of suspicion and control that was informal but no less real and that had to have actual examples of honored "Eyes" as incentives to encourage others. Aeschylus, Herodotus, Aristophanes, and Ktesias spoke of a singular Eye because that was convenient for their dramatic and narrative purposes. They need not be taken to mean, however, that there was one and only one Eye.

Hirsch would like to show that Xenophon's careful knowledge of Persian ideas and practices created in his thinking a personal synthesis of Greek and Persian traditions. This would be a genuinely interesting case to elaborate, but, apart from some easy correlations made between Xenophon's experience with Cyrus the Younger and his presentation of Cyrus the Great, Hirsch never really does so. His book winds up being an honest, welcome, and sometimes-useful correction of mistaken views about Xenophon but does not leave the (perhaps too-greedy) reader with a focused understanding of what finally integrates Xenophon's orientalism. One could begin by observing that Xenophon's "friendship of the barbarians" may have earned him a lengthy exile from the Athens that had executed Socrates.

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BERYL RAWSON, editor. *The Family in Ancient Rome: New Perspectives*. Ithaca: Cornell University Press. 1986. Pp. 279. $27.50.

In the current decade historians in Australia, New Zealand, the United States, and, to a lesser extent, Cambridge, England, have been working to reshape our picture of the family in classical antiquity. The volume under review is the result of a conference held in 1981 at the Australian National University in Canberra.

Three of the eight papers exploit sepulchral inscriptions in pioneering studies of children of various legal status. In "Children in the Roman *Familia*" Beryl Rawson examines the various functions of the *familia* as a surrogate family for lower-class children. *Alumni* (foster-children) are distinguished from *vernae* (slaves born at home). The sex ratio of both groups was drastically skewed in favor of boys. In "The Status of Children in Mixed Marriages" P. R. C. Weaver studies children born to parents of whom one was a slave and the other a Roman citizen. Weaver's article demonstrates the complexities of ancient slavery, in which free men and women found that slaves of the emperor were attractive marriage partners. In effect, the legal discouragement of hypergamy for male slaves reinforced a gender hierarchy, so that a free wife would not enjoy a higher status than her husband. But the epigraphic evidence belies the intent of the law. More than three-quarters of the wives in the *familia Caesaris* were not slaves when their names were inscribed. Keith R. Bradley continues his work on wet-nursing in the Roman empire by focusing on the city of Rome in "Wet-nursing at Rome: A Study in Social Relations." Despite the subtitle, Bradley's article, like the other two on children, avoids psychoanalytic speculations on the effects of wet-nursing, exposure of infants, and the dissolution of families by sale or death.

Three papers are based on legal evidence. Two contributions by J. A. Crook, "Women in Roman Succession" and "Feminine Inadequacy and the *Senatusconsultum Velleianum*,"—written in the offhand, noncommittal style affected by contemporary legal historians of antiquity—review earlier work on these subjects. In contrast, in "*Patria Potestas*" W. K. Lacey essays a major theoretical flight, proposing that the Roman state was a macrocosm of the Roman family. Lacey's chapter opens with a concise discussion of the differences betwen the Greek and Roman family. I hope this volume will demonstrate to historians who make sweeping generalizations about the development of the European family that the Greeks differed from the Romans and that family structure varied according to class.

The two remaining chapters are based on literary evidence. In "Family Finances: Terentia and Tullia" Suzanne Dixon discusses the economic status of Cicero's first wife and daughter. Unlike the other authors in this volume, Dixon is able to demonstrate change over time. The legal sources not only show a development in views concerning the relationship of women to matrimonial property but also indicate that within Cicero's family women's control over property was affected by the life cycle and by external events. In the last essay Jan Blayney offers a brief survey of some of the major theories of conception from the pre-Socratics to Galen. The volume ends with a bibliographic essay, in which the latest item cited appeared in 1982.

This volume contains highly specialized studies that, taken together, do not comprise a comprehensive picture of the Roman family. Some of the major subjects central to such a study (such as adoption, the household economy, regional variation, and linguistic reflections of family relationships) have been omitted. The editor, however, has written a

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lengthy introduction that is the best synthesis currently available on the Roman family.

The traditional notion of the Roman family was based primarily on legal evidence and studies of the elite. The family was seen to be extended, multigenerational, frequently broken by divorce, and characterized by the domination of an all-powerful *paterfamilias* over children as well as slaves. By showing that, especially among the lower classes, families were small and conjugal, children could be cherished, and marriages often were of long duration, this new volume serves as a much-needed corrective to the older monolithic view.

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R. DEVELIN. *The Practice of Politics at Rome, 366–167 B.C.* (Collection Latomus, number 188.) Brussels: Latomus. 1985. Pp. 354. 1,575 F.

Prosopography, tracing influences of family ties on political factions, has stimulated Roman studies for a half century. But the widespread acceptance of this interfretted explanation for so much in Roman elections also causes scholars such as R. Develin to caution against excess and oversimplification. In this work Develin extends his previous studies to a full appraisal of two centuries, 366–167 B.C. He unequivocally states his intentions of breaking the hold of factional suppositions based on inadequate information and faulty means of reaching conclusions on this period. He admits but does not elaborate the greater validity of prosopography for the later Republic.

Develin judges that the sources, primarily Polybius and Livy, provide scant evidence for factions. The *Fasti Consulares* are more reliable, but the *Fasti* provide electoral results only for consulships and omit lower offices, which Develin considers more significant for factions. He also questions the methodology of reconstructing factions on the bases of state policies and charges that modern historians have artificially constructed factions around certain policies, then found individuals to fill the factions. Rather, responsible senatorial committees, not factions, evolved policies to meet recognized needs of the state.

Develin presents reasonable alternatives to factional explanations of state policies. The needs of war or diplomacy brought forward the ablest and most cooperative men. A statesman could rise above his personal sympathies to respond to state needs. Disagreements about policies could be honest. And circumstances, not family ties, accounted for dictatorships, delayed elections, and interregnums. Even new provinces and colonies were not calculated for possible clients but were undertaken by Senate committees showing no fear of manipulated client votes.

Backing these claims with meticulous reappraisal of the sources, Develin details the content and reliability of the evidence. He describes the organization and work of the Senate, assemblies, offices, and religious institutions and admits their domination by patricians and equestrians. He particularly stresses the curule aedileship as the critical office to win, since curule aediles normally progressed to the consulate. They might face delays, even electoral defeats, but most curule aediles in time were consuls. Hence, any factional manipulation must occur at this stage, long before significant state policies could be formulated for a consular year.

Develin readily acknowledges that family names and experience gave great advantages in elections. Family consultations designated appropriate candidates and timed their advancement. Clusters of family names in offices reflect large *gentes*, and the generations of family candidates could bring names forward in regular sequences. Candidates normally followed the traditional patrician attitudes and patterns. Yet, achievement in office was essential, and able new men could advance. Develin concludes, after tracing the leading families of his two centuries, that factional interests almost never determined elections.

Develin's book contributes valuable and challenging reinterpretations and arguments. He has thoroughly sifted both ancient and modern bibliographies. His presentation suffers from some repetition and some diffuse discussion of tangential topics. Objections will be raised over a number of his judgments. But, ultimately, Develin poses fundamental and provocative questions about the realities of Roman politics and prosopography.

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MARY BEARD and MICHAEL CRAWFORD. *Rome in the Late Republic*. Ithaca: Cornell University Press. 1985. Pp. 106. $22.50.

This brief book was written to encapsulate for British students the beliefs of modern historians (especially British historians) on the culture, society, and politics of the later Republic. It is likely, however, to attract a wider readership. For nearly two generations British historians have been virtually preeminent in interpreting this troubled and exciting period, and Mary Beard and Michael Crawford have added so many bibliographic references to their summary that they provide a sort of progress report (albeit a partisan one) on contemporary British thinking.

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The authors presume a general knowledge of the sequence of events that led to the Republic's collapse; instead, they provide an "analytical description" that "attempts to demonstrate how a whole network of factors are closely interlinked, how they cumulatively alter the character of the political process and how that changed character adds new elements to the network" (p. 4). The book unfolds as a series of loosely coordinated chapters on late republican "culture," religion, politics, and imperial governance. The authors lack space to expound their argument in detail and instead rely mainly on broad assertions illustrated by well-chosen anecdotes.

Although the book therefore does little more than sketch a theory regarding the dynamics of late republican history, even in this limited respect many sections are highly interesting. The authors, pursuing quasi-sociological modes of analysis introduced by Moses Finley and Keith Hopkins, examine the late Republic from a broad perspective. "The changes in late Republican society should not be seen just as the causes of the breakdown of the specifically Republican political system; they were also parts of the *development* of Roman society, which underwent no clean break between the 'fall' of the Republic and the 'advent' of empire" (p. 71).

In accord with this well-justified observation, the authors isolate important instances of social coherence and continuity not only in the social and political system (pp. 62–67) but even, somewhat more surprisingly, in the traditional state religion (pp. 30–39). By contrast, the political transition to the principate, "a system monarchical in all but name" (p. 1), was precipitated by an absolute increase both in "the value of the [political] prizes to be won" and in "the intensity of competition for high office" (p. 69).

Although much of their theorizing is at least plausible, the argument is too concisely presented to carry complete conviction. Numerous difficulties remain to be explicated on a larger scale. For instance, the authors adopt a view of Roman society in which "status" is firmly elevated above "class" as the basic social unit (pp. 40–49); yet they equally firmly reject traditional prosopographic approaches and argue that "it is . . . difficult to *make sense* of Roman politics without incorporating issues of ideology that transcended family allegiance" (p. 68). But their account is unclear on what these ideologies are supposed to have been and why they apparently lacked a material foundation. Indeed, material aspects of late republican history—demography, economics—are largely ignored. The authors entirely omit discussion of events that their theories might seem ill designed to handle, such as Spartacus' slave rebellion or the attempted coups of Aemilius Lepidus and Catiline.

For an American audience, this book is especially well suited as a provocative introduction to the late Republic for undergraduates in courses on classical civilization. Fortunately, its American publisher has now issued a much cheaper paperback edition.

BRUCE W. FRIER
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JOAN M. PETERSEN. *The* Dialogues *of Gregory the Great in Their Late Antique Cultural Background.* (Studies and Texts, number 69.) Toronto: Pontifical Institute of Mediaeval Studies; distributed by E. J. Brill, Leiden. 1984. Pp. xxiv, 227. f. 51.

Modern historians hail Pope Gregory the Great as one of the seminal figures in early medieval history, justly renowned for his initiative in the conversion of England, his leadership in an Italy divided between Lombards and Byzantines, and his sermons, letters, commentaries, and pastoral advice. But historians have also been somewhat embarrassed by Gregory's *Dialogues*, because in them the great man recorded without criticism the many miracles performed by contemporary Italian saints. In order to sidestep these misgivings about Gregory's credulity, Joan M. Petersen adopts a literary and typological approach to the *Dialogues*. Informants or participants reported most of these miracle stories to Gregory; then, according to Petersen, he tended "to interpret his material in biblical terms or to impose upon it a literary form derived from other sources" (p. 8).

The suggestion that literary conventions and spiritual implications took priority over historical veracity leads to the heart of the book, which is a meticulous and learned analysis of the possible literary antecedents and parallels for the anecdotes in the *Dialogues*. The Bible was most important in shaping Gregory's spirituality. Long meditation had taught Gregory to interpret biblical events and individuals in allegorical terms applicable to contemporary situations; in the *Dialogues* Gregory now refined that approach by presenting holy men simply as "portrait types of Christian sanctity" (p. 54). Other saints' lives, however, apparently were less directly influential. After an extended survey of most of the extant Latin hagiography from the fourth, fifth, and sixth centuries, Petersen can conclude only that Gregory was familiar with a Latin translation of Athanasius's *Vita* of St. Anthony (and then perhaps secondhand) and Sulpicius's *Vita* of St. Martin. Even when Gregory shared literary *topoi* with other Latin hagiographers, the purpose of his stories was different. Whereas his contemporary, Gregory of Tours, was more concerned with promoting the authority of bishops as the local guardians of saints' tombs, Gregory intended to communicate a message of spiritual edification and moral insight.

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For all the carefulness of the analysis many of these conclusions are thus highly conjectural. Although Petersen constructs a web of interlocking literary parallels, she too often relies on a hypothetical familiarity with common sources, common motifs, and widespread traditions for which no direct evidence exists. This tendency is most apparent in her analysis of the relevance of Greek hagiography. Because Eastern hagiographers apparently had drawn on the same motifs and traditions, she postulates the existence of a "general Mediterranean cultural heritage" (p. 25). Since Gregory's Greek was inadequate for fluent reading of Greek texts and since Latin translations of all the Greek hagiography that Petersen considers relevant were not available, only this cultural *koine* can explain these Eastern influences on Gregory. As a result, Petersen also suggests that Gregory's emphasis on the miracles of living holy men seems to reflect an Eastern viewpoint, in distinction from the prevailing Western emphasis on dead saints and their relics as miracle workers. This final contrast will, again, remain tentative, until more sociologically oriented studies of specific cults and regions become available. But Petersen's finely tuned study rightly stresses the importance of Gregory's *Dialogues* in the interaction between Latin West and Greek East, and her book now allows modern scholars to share the appreciation of the Byzantines, for whom Pope Gregory the Great was, simply, "Gregory of the *Dialogues*."

RAYMOND VAN DAM
University of Texas

ROBERTO BONINI. *Introduzione allo studio dell'età giustinianea*. 4th ed., rev. (Studi e Materiali per gli Insegnamenti Storico-Giuridici, number 11.) Bologna: Pàtron. 1985. Pp. 132. L. 12,000.

The appearance of the fourth edition of this book since initial publication in 1977 attests to its lasting value. So far as I can establish, not a great deal has been altered through the successive editions, and, since the book has not circulated very much on this side of the Atlantic in its earlier issues, it is as good as a new work in the form reviewed here.

Roberto Bonini is an established scholar in the history of law in the grand lineage of the Italian tradition in this field and in the Bologna tradition in particular. Most of his numerous publications have been aimed at students, as textbooks or as resources in legal study, and have mainly concentrated on the history of Roman law. The author's expertise together with the nature of the series in which this book is issued make clear, far more than the title does, the subject of the book. It is not, as might first be expected, a survey of Justinian's age (A.D. 527–65) as an historical epoch. Rather, it is directed to the student of Roman law and attempts to place Justinian's pivotal legal codification in the setting of his era.

Accordingly, this short volume is divided into seventeen concise and pithy sections, or *paragrafi* as the author calls them. Following the introductory section, the next two sketch Justinian's life and reign and then attempt to distinguish between two basic periods of legislative activity within the reign, dividing about the year 541. The ensuing eight sections are devoted to the four major segments comprising the *Corpus juris civilis* (one to the Codex, three to the Digest, one to the Institutes, one to the revised Codex, and two to the Novels—one for each of the legislative phases of the reign). These sections examine the character, contents, and, above all, the methodology of these legal compilations. The texts themselves are analyzed to discern the rationales and techniques that went into their assemblage.

The remaining six sections discuss specific themes. Particularly useful is a review of the texts in the compilations, which reveal the contemporary understanding of the imperial office and sovereign power itself. All students of late Roman history would do well to peruse this section. Two more sections discuss the character and legal procedures inherent in the judicial life of Justinian's age. Another two sections provide a prosopographic review of "collaborators" and legal experts, identified variously by our sources as involved in the compilation projects or the legal life of the era. A closing section surveys the literary sources that survive for this period, to guide those who wish to go beyond the specific legal texts themselves in seeking contemporaneous evidence.

Bonini's style is dense and somewhat technical at times, so that this book does not make for easy skimming. Still, the author thoroughly digests the state of knowledge of his subject. He offers no new interpretations nor any ventures in theorizing, but he indicates areas of scholarly debate on given issues. Unfortunately, he provides no footnotes or citations along the way. The only guidance provided to the literature is a supplemental bibliography, hardly over six pages long (pp. 123–29), giving a cursorily critical review of selected general and specialized publications. It is, in fact, much too cursory and is difficult to use in relation to the specific topics discussed in the text. The reader needs a better guide into the intricacies of this field's resources.

Although clearly intended to offer historical and general perspective to the legal specialist who approaches the Justinianic age as a watershed in the history of law, this book can also serve to guide the generalist, or the reader whose outlook is historical, into one of the era's most important aspects. Accordingly, this book merits wider circulation than its present format and language allow. Especially if its

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citational and bibliographical apparatus were expanded, it would justify translation from Italian.

JOHN W. BARKER
University of Wisconsin

MEDIEVAL

PIERRE DUHEM. *Medieval Cosmology: Theories of Infinity, Place, Time, Void, and the Plurality of Worlds.* Translated by ROGER ARIEW. Chicago: University of Chicago Press. 1985. Pp. xxxi, 601. $35.00.

Pierre Duhem, the distinguished historian of science, began to publish his *Système du monde: Histoire des doctrines cosmologiques de Platon à Copernic* in 1913; the later volumes were published posthumously at intervals up to 1959. A pioneer in the study of medieval science, Duhem rescued it from the contempt in which it was generally held and demonstrated its achievements and its contribution to the science of the Renaissance and to the scientific revolution. The book, presenting classical and medieval physical theory against its background of philosophy and theology, was the culmination of his life's work. His equally distinguished opponent, Alexandre Koyré, recognized it in 1956 as irreplaceable and indispensable, and the present translator, Roger Ariew, asserts that "twenty-five years later, Koyré's claim is still valid, and it can be extended" (p. xix). This monumental work has never been translated into English, and the current growing interest in medieval science clearly shows a need to make its riches widely available. Ariew seeks to fill this need by issuing the most important parts of the *Système du monde* in a one-volume translation.

Cosmology, which can be simply described as the theory of the universe, can be variously defined, so latitude of interpretation is possible. Duhem understood it broadly to mean physical theory in general. Ariew chooses to understand it more narrowly. This book is, in his own words, "a selection of translations from the *Système du monde*, dealing with cosmology proper and restricted to the Middle Ages" (p. xx). It comes mainly from Duhem's volumes VII and X, with a little from volumes I, VIII, IX, and from Duhem's *Etudes sur Léonard de Vinci* series II. (An unfortunate misprint in the table of contents renders Duhem's volume VIII, pp. 7–60 as VII, pp. 7–60).

What does Ariew mean by "cosmology proper"? His subtitle indicates it to be "theories of infinity, place, time, void, and the plurality of worlds." Other scholars might have made a somewhat different selection. Topics treated by Duhem and omitted by Ariew include astronomy, astrology, Aristotelian physics, the shape and size of the universe, tides and the relationship of sea and land, motion theory, form latitudes, trends in scholasticism, and philosophical (especially Neoplatonic) influences on medieval philosophy. Ariew's omissions are reasonable, given the size of the book, the limitation of period, and the advancement of some topics (such as medieval astronomy and motion theory) by writers after Duhem. He wisely concentrates on topics in which Duhem's work has not been superseded. He includes Duhem's controversial thesis that the ecclesiastical condemnation of 1277 encouraged medieval science.

Ariew has skillfully performed an important task. The treatment of medieval cosmology within a single volume is convenient, and the book's arrangement presents the subject matter clearly. The translation is usually readable and accurate. Many medieval authors, both well-known and obscure, are represented, and Duhem's analysis of their views is given. Stanley L. Jaki's foreword deals with Duhem's life and work. Ariew's preface usefully sketches in the background of Aristotelian cosmological doctrines. Duhem offered no bibliography, but Ariew supplies a list of medieval works cited by Duhem, with authors' dates and with references to modern editions and English translations, a selected bibliography of Duhem's works, and a list of relevant works published after Duhem.

So Ariew ensures that "the reader is not only treated to medieval cosmology but also to Duhem's analysis" (p. xx), and he admirably fulfills his aim "to remain as faithful as possible to Duhem . . . to track down Duhem's original sources and . . . to indicate where the interested reader might find these documents" (p. xxiv). His book is valuable in itself and could also serve as an introduction to the further treasures of erudition that Duhem has to offer.

NORMA E. EMERTON
Cambridge University

MICHAEL J. ENRIGHT. *Iona, Tara, and Soissons: The Origin of the Royal Anointing Ritual.* (Arbeiten zur Frühmittelalterforschung, number 17.) New York: Walter de Gruyter. 1985. Pp. viii, 198. $51.80.

Michael J. Enright contributes importantly to the extensive historiography on the origin of Pepin III's anointment as *rex Francorum* in 751. The work also brings the history of seventh- and eighth-century Ireland into sharper focus. Throughout the book we find astute conceptualization, skillful textual elucidation, and firm command of the secondary literature.

Although Ireland experienced no royal unction before 751, it nevertheless produced a mature ideology of royal anointment between 690 and 725. The ideology was transferred to Pepin's court by the

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Celtic ecclesiastic Virgil of Salzburg some time before 751. Application occurred because elements of the Celtic formula of legitimation conveniently justified Pepin's deposition of the Merovingian line to an angry Frankish aristocracy and gave Pepin a Germanic sacral quality that transformed him from Carolingian noble to Frankish monarch, "a new born sinless king who could guarantee victory and prosperity in the minds of the folk" (p. 165). Such is Enright's very original hypothesis.

His argument for the existence of a Celtic ideology of royal unction rests on his novel tandem analysis of two early eighth-century Irish texts, the chapter on royal ordination in Abbot Adomnán's *Vita Columbae* and select entries in the *Collectio canonum Hibernensis*. Enright demonstrates that the texts contain a well-developed Christian ideology of royal unction predicated on biblical understandings posed in 1 Kings. Also compelling is Enright's characterization of Adomnán and two cocompilers of the *Collectio* (Ruben of Dairinis and Cú Chuimne) as a task force devoted to producing the new ideology for use as propaganda by the *paruchia Columbae*. The *paruchia Columbae*, headed by Adomnán, needed new influence over the Uí Néill monarchy to tighten the traditional kinship-political linkage of the *paruchia* with the dynasty and to oppose the rising power of Armagh.

Enright firmly establishes Virgil as the agent of the ideology's transmission: he was a student of the *Collectio*, a close associate of the *paruchia Columbae*, a protégé of Pepin—even a member of his inner circle—and a colleague of Fulrad, Pepin's agent in the critical negotiations of 750 with Pope Zachary, which launched the coronation. Through Virgil, Adomnán's ideology of unction must have been known in detail by Pepin and Fulrad.

Enright's assertions about the application of the ideology in the anointment of 750 and about the derivation of the anointment from exclusively Irish sources will probably receive a mixed response. For example, most readers can agree that Pepin applied the Adomnán-Virgil model because its disapproval of a generic *rex iniquus* coincided with Pepin's urgent need to justify his deposition of a real *rex iniquus*, Childeric III, but readers inclined to see a strong papal hand in Carolingian affairs from at least mid-century may reject the denial of those portions of Arnold Angenendt's persuasive thesis about the development of unique papal-Frankish spiritual and familial bonds that imply a Petrine role in the anointment. Students of the Anglo-Saxon Continental mission may also be distressed: Enright largely ignores Boniface, an oddity since Boniface himself may have performed the unction.

In sum, despite some significant problems, Enright gives us a provocative work. Celticists must now reappraise Adomnán's world, and Continental specialists will have to accept Irish thought as one of the sources of the anointment of 751, whatever other influences may have been at work.

JAN T. HALLENBECK
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KURT RUH. *Meister Eckhart: Theologe, Prediger, Mystiker*. Munich: C. H. Beck. 1985. Pp. 208. DM 42.

The explosion of interest in Meister Eckhart over the past decade has produced both new translations and some impressive studies of particular topics. Thus far a good general work that would not only introduce the major themes of Eckhart's writings to the public but also situate them in their historical context has been lacking. Now Kurt Ruh, the dean of German Eckhart scholars, has gone a long way to filling the need in this fairly brief, but impressive book.

Ruh adopts a biographical approach; his ten chapters follow Eckhart's life and writings from 1294, the first established date in his career, through to his death and posthumous condemnation. Building on Joseph Koch's studies of Eckhart's life, Ruh brings new precision to the chronology of the German Dominican's writings and the evolution of his thought through a combination of careful philological and intellectual detective work. Although he admits that a number of his arguments must remain hypothetical, in general the picture he presents is both convincing and more complete than that of earlier investigators.

After completing his studies in Paris, Eckhart returned to Germany to serve as prior at Erfurt and vicar of Thuringia. Here he composed his earliest vernacular work, *Die rede der unterscheidunge*, as well as the mystical poem "Granum sinapis" and its accompanying Latin commentary whose ascription to Eckhart Ruh defends in chapter 4. The Dominican's first period as teaching master at Paris (1302–03) saw the production of the "Parisian Questions." From 1303 to 1310 he was provincial of the newly created province of Saxony from which time the sermons found in the collection "Paradisus animae intelligentis" date. Eckhart was back in Paris from 1311 to 1313 for an unusual second teaching period, and at this time his thought deepened, as was reflected in the Latin works belonging to the vast and uncompleted *Opus tripartitum*. Here he also came into contact with the mystical piety of the Beguines, especially the condemned Marguerite Porete (see chapter 7). From 1313 to 1323 Eckhart was at Strasbourg, preaching extensively, especially to female religious. These late sermons, as well as the *Liber benedictus*, which Ruh dates to 1318, are among Eckhart's most powerful works. He moved on to Cologne, where he was attacked by the Inqui-

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sition in 1326. Tried first at Cologne, then at Avignon, he was eventually condemned in 1329.

Although some parts of Eckhart's *ouevre*, especially the extensive Latin commentaries, could be given more attention, Ruh's account is fair and his knowledge of Eckhart is profound. A number of Ruh's central contentions, especially his insistence that, by the thirteenth century, Latin could no longer serve as an adequate vehicle for mystical expression, though debatable, are on the whole convincing. The book suffers to some extent from its, at times, questionable handling of central elements of Eckhart's speculation. The most striking example is the lack of attention given to the dialectical character of Eckhart's doctrine of God. Ruh's presentation of Eckhart's teaching on analogy is not adequate, and his insistence that an "Ineinandergreifen von Analogie und Univozität" (p. 85) is sufficient to explain Eckhart's understanding of the relation of God and creation does not do justice to the major dialectical texts found throughout the *Opus tripartitum*.

BERNARD MCGINN
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LOUIS GRODECKI and CATHERINE BRISAC. *Gothic Stained Glass, 1200–1300*. Ithaca: Cornell University Press. 1985. Pp. 278.

Few recent historians of medieval art have had interests as wide ranging as those of Louis Grodecki. At his death in 1982 he left a large body of key publications concerning architecture, sculpture, and stained glass. Stained glass particularly interested Grodecki, and he can be credited, both through his publications and his teaching, with initiating the recent dramatic increase in studies of that medium.

The book reviewed here, a sequel to *Le Vitrail roman* (1977), was incomplete at the time of Grodecki's death. He had written the first three chapters but had only outlined the remaining two chapters and the catalogue. The book was finished by Catherine Brisac, Grodecki's student and assistant, with help from others.

The book begins with a chapter defining thirteenth-century stained glass, discussing various aspects of style, iconography, production, and patronage. The next three chapters deal with French stained glass in the first quarter, the second quarter, and the second half of the thirteenth century, respectively. A fifth chapter surveys European developments outside of France. This chapter is followed by a brief conclusion, a catalogue of major examples of thirteenth-century stained glass, and a somewhat perfunctory scholarly apparatus.

The book is a flawed production. Irritants abound. Specialized terms, critical to the study, are often introduced without adequate definition, such as, for example, "Romanesque," "Late Romanesque," "Proto-Gothic," "Gothic," and "Mannerist." French words and phrases are usually translated (for example, *terre à vitraux* [p. 153]), but, oddly, Latin and German terms are not (*Muldenfaltenstil* [p. 91]). The sparse use of footnotes frequently creates a frustrating lack of specificity. What American museum, for instance, houses panels from the window at Amiens depicting the life of Saint Eligius (p. 116)?

Overall, the book is marred by a lack of coordination, apparently underlining its cooperative authorship. The translation compounds the problem, for it is frequently too literal, resulting in stilted prose and occasional ambiguity.

Despite these problems, the book has merit. The illustrations are judiciously chosen and excellent in quality. Some of the monographic sections concerning the glass at specific monuments, such as Chartres, offer reasonable summaries of the current state of research. In a broader sense, regardless of flaws, the book does fill a void. To date no other study has attempted such coverage.

This book is difficult to recommend. The intended audience of the book is unclear. The rather lavish format suggests a Christmas book, if not a coffee-table book. Yet the text is anything but popularized. If the book is intended for students, it misses the mark. It assumes too much knowledge from the reader. Ultimately, the book will only serve specialists and, even then, not as a scholarly tool but rather as a farewell to a great colleague. The desire to publish this work is both understandable and laudable. Unfortunately, it does not truly reflect Louis Grodecki's high standards.

ERIC G. CARLSON
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GERHARD FRITZ. *Die Geschichte der Grafschaft Löwenstein und der Grafen von Löwenstein-Habsburg vom späten 13. bis zur Mitte des 15. Jahrhunderts*. (Forschungen aus Württembergisch Franken, number 29.) Sigmaringen: Jan Thorbecke. 1986. Pp. 396. DM 48.

The church's christianization of marriage limited the inheritance rights of bastards, but, in Germany during the High Middle Ages, bastards of acknowledged paternity were often assigned a rank one rung beneath their fathers' in the *Herrschildordnung*—that is, the sons of nobles became ministerials, the sons of ministerials, knights. Gerhard Fritz has studied the descendants of a royal bastard,

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Albrecht of Schenkenberg, the son of Rudolph of Habsburg and a lady known only as Ita, and the county of Löwenstein, which Rudolph purchased and granted to Albrecht in 1282–83. In addition, Rudolph enfeoffed Albrecht with the imperial tithes in Heilbronn, which provided the counts with half or even two-thirds of their income, and arranged for Albrecht's marriage to a Bolanden heiress, but her Rhenish lands were soon lost. Albrecht's establishment in the Neckar Valley was part of Rudolph's plan to secure the duchy of Swabia for the Habsburgs, but the Löwensteins were rapidly reduced to insignificance after the Habsburgs lost the crown. The lineage declined for a number of reasons: the loss of royal favor; pressure from the Löwensteins' more powerful neighbors, Baden, Württemberg, and the Palatinate; long minorities of heirs; financial mismanagement; and the late medieval agrarian crisis. The counts were forced to pledge half of the county to the Palatinate in 1382 and to sell the other half in 1441, although the Palatinate did not take actual possession until 1453. The dynasty ended with the death of its last member, a cathedral canon, in 1464.

Fritz's study is a model of German thoroughness. The first part contains a history of the county, a detailed list of the counts' holdings, ministerials, and vassals, a genealogical study of the lineage, maps of the counts' lands, and pictures of the counts. I found the reconstruction of Albrecht I's features, based on a forensic examination of his skull, particularly intriguing. The second part consists of a register of 570 documents, most of which have been published or summarized elsewhere. The register, however, is not complete, especially after 1350. It is hard to believe that anyone will ever need to study the Löwensteins again.

Still, the book is slightly old fashioned. I wish that Fritz had applied Karl Schmid's techniques for studying the self-consciousness of a lineage to the Löwensteins. Such an approach might have provided additional answers to Fritz's question about the degree of Albrecht's independence from his father and royal half-brother. Remarkably, Albrecht named his eldest son not after Rudolph but after his Bolanden grandfather, and the son who continued the lineage, Nicholas, bore a name that appears among neither the Habsburgs nor the Bolandens. Most likely Nicholas was named after the patron saint of the parish church of Löwenstein, and the choice of this name was, like the Habsburgs' adoption of the leading name of their Babenberg predecessors, Leopold, a deliberate attempt to associate the dynasty with its new home. Moreover, the Löwensteins never employed, as far as I can tell, the name Habsburg. Although the Habsburg connection aroused scholarly interest in the Löwensteins, their real significance is, as Fritz pointed out, as an example of a late medieval dynasty that failed to establish a successful territorial state.

JOHN B. FREED
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WILLIAM CHESTER JORDAN. *From Servitude to Freedom: Manumission in the Sénonais in the Thirteenth Century.* (The Middle Ages.) Philadelphia: University of Pennsylvania Press. 1986. Pp. viii, 149. $20.00.

This essay on serfdom in medieval France is particularly welcome as a clear presentation in English of the process of enfranchisement, one of the most significant social events in thirteenth-century France. William Chester Jordan has cut through the jumble of an overly legalistic historiography by focusing on a well-documented case in the city of Sens—the manumission in 1257 of 366 men and women attached to the abbey of St. Pierre-le-Vif. With the aid of tax lists, charters, chronicles, and obituaries, Jordan is able to identify a number of individuals named in the franchise, such as the wealthy Pierre Ribaut and the leather merchant Jean Mouflet as well as several persons of modest means.

The story revolves around Abbot Geoffroy, whose ambition to rebuild the abbey church led him to reassert long-forgotten claims over the abbey's men resident in its burg at the edge of Sens and over a few others who had migrated to nearby villages. The abbot's men resisted what they regarded as demeaning obligations, taxes, and restrictions that marked them as inferiors compared with their neighbors in Sens, who had enjoyed a royal franchise and a self-governing commune for several generations. Through the mediation of the royal bailiff of Sens, the abbott lifted the head tax (*taille*), labor service (*corvée*), tax on out-marriages, and restrictions on property transfers and entrance into clerical orders. The franchised agreed to pay 6000 pounds over twelve years, a sum, Jordan finds, that severely strained their resources and even pushed some into destitution. Unfortunately, we do not learn about the larger consequences of manumission in and around Sens, where perceptive lords, notably the king and the count of Champagne, had widely commuted the petty exactions reclaimed by the abbot into lucrative annual taxes based on wealth. Clearly the motives of those lords—to encourage prosperous tax-paying communities—were not those of the abbot.

Jordan rightfully avoids the compromised term serfdom, but his use of "servitude" is an equally misleading rendition of *servitus*, which included all types of services owed by various social classes. Moreover, servitude in English connotes personal dependency, whereas the weight of recent research

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suggests that the exactions commuted by franchises arose from residence and tenure rather than from personal bondage. These are, of course, difficult issues. If this book has not solved them all, it has the merit of posing a fresh reading of an important question in medieval social history.

THEODORE EVERGATES
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JACQUES PYCKE. *Le chapitre cathedral Notre-Dame de Tournai de la fin du XI^e^ à la fin du XIII^e^ siècle: Son organisation, sa vie, ses membres*. (Université de Louvain, Recueil de Travaux d'Histoire et de Philologie, sixth series, number 30.) Louvain-la-Neuve: Collège Erasme or Nauwelaerts, Brussels. 1986. Pp. xxix, 391. 1,200 F.

This book is an exhaustive study of the personnel and the administrative machinery of the cathedral chapter of Tournai during the twelfth and thirteenth centuries, a period during which, the author argues, the chapter took the form it retained until the revolution. Jacques Pycke started working on the exceptionally rich archives of Tournai for his doctoral thesis and now knows them thoroughly. This book bristles with erudition, including numerous maps, material presented in charts and tables, several *pièces justificatives*, and lists of all the capitular officers during the period as well as the other four hundred or so members of the chapter whom he has identified.

Semi-independent of the bishops and nearly as important in diocesan administration, cathedral chapters were a vital part of the ecclesiastical hierarchy in the High Middle Ages. Yet scholars have studied these chapters very little and have concentrated instead on the bishops themselves or on monasteries and colleges of canons regular. In part this is a problem of sources. Few cathedral chapters had extensive archives before the late thirteenth or fourteenth centuries. Thus, most studies of medieval cathedral chapters have only covered the late Middle Ages, by which time, Pycke argues, the most important developments had already taken place. Pycke fills this gap for one diocese with his detailed analyses of the selection of canons, their social and ecclesiastical background, the function of the capitular offices, and the process by which capitular property was divided into prebends.

But the total immersion in the capitular archives of Tournai, one of the strengths of this book, is also one of its weaknesses, for Pycke gives little context for most of the information he presents. The chief example is his almost complete exclusion of the bishops of Tournai from these pages. He never discusses the election of new bishops, which was one of the principal functions of cathedral canons, nor deals with the advancement of canons to become bishops, even though, one assumes, based on studies of other dioceses, most bishops had once been cathedral canons. Similarly, although he demonstrates that at least two-thirds of the canons whose origins are known were of noble or knightly families, he only mentions northern French and Flemish dioceses for comparative purposes and does not note that this pattern was probably universal. He gives no real discussion of the significance of the change he discovered from the predominance in the chapter of noble clerics of the region to that of noble clerics of other regions between the twelfth and thirteenth centuries. Nor does he discuss the results of the growing frequency of holders of university degrees in the chapter. Thus, readers must be prepared to make their own comparisons and draw their own conclusions from the mass of information presented here.

Anyone interested in medieval ecclesiastical government should read this book. If scholars of other areas are inspired by its example to undertake similar studies, it may someday be possible to write, for the first time, a general history of medieval cathedral chapters.

CONSTANCE B. BOUCHARD
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JIM BRADBURY. *The Medieval Archer*. New York: St. Martin's. 1985. Pp. x, 198. $29.95.

Jim Bradbury has written a mildly revisionist book about medieval warfare. As the title indicates, the goal is to redress the balance against the medieval and also modern prejudice in favor of the knights and chivalry. It is highly readable; one notes the contribution of such a learned popularizer as Richard Barber, not to mention sessions with fellow scholars in the Marquess of Anglesey pub. The revision is kindly but firm. The formidable voice of R. A. Brown is to be heard here, which gives the Anglo-Norman chapters a special piquancy. The bibliography includes most modern writing in English and French, together with about three hundred printed sources, nine-tenths of them narrative and some archaeological. This is not the stuff of fundamental scholarship, but it does leave us with enjoyable Whiggish targets for students to shoot at.

A chapter on Robin Hood, which is intended as a bridge to a study of later medieval warfare, might better have been incorporated in the final chapter, "The Archer in Society," for it is too inconclusive to help the main thesis. The next chapter is on the advent of the longbow. Bradbury rejects J. E. Morris's view that the Welsh longbowmen, combined with cavalry, made England supreme on the

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battlefield. The bow itself underwent no dramatic change; rather, Edward III, in his Scottish wars, developed new tactics that were brought about the victories of Crecy, Poitiers, and Agincourt.

For the Hundred Years' War Bradbury follows A. H. Burne, though not without criticism. On the "herce," on the the long-delayed French response, and on the technology of bows, crossbows, and guns, he has valuable words, but I am afraid that here, as in the chapter "The Archer in Society," the crusade for the common man becomes somewhat diffuse and inconclusive.

MICHAEL R. POWICKE
University of Toronto

ARCHIBALD R. LEWIS and TIMOTHY J. RUNYAN. *European Naval and Maritime History, 300–1500*. Bloomington: Indiana University Press. 1985. Pp. xiii, 192. $22.50.

Students of imperial and maritime history will gain rich perspective from this comprehensive survey of naval and maritime rivalry during the millenium that preceeded the great age of European trans-Atlantic expansion. Based on an impressive array of monographs and major works, fresh translations of contemporary chronicles, and reports from modern underwater archaeological sites, this highly informed account explores the successive, intertwined fortunes of late Roman, Byzantine, Muslim, Latin, Irish, Frisian, Viking, British, Hanseatic, and Iberian maritime expansionists. It proves a rich panorama indeed, notwithstanding the authors' opening admonition regarding remaining lacunae and the likelihood that future archaeological finds will shed new light on existing data on medieval ship construction and equipment. Arrestingly enough, however, Archibald R. Lewis and Timothy J. Runyan observe, on the technical level, that "ship types were not only constantly modified in myriad ways through the passage of time, but tended to blend into one another during the same period as well" (p. xii).

Provided with a variety of technical illustrations, this collaborative work provides clear evidence of the fruitful multidisciplinary approach to maritime history encouraged in recent years by four specialized associations, the Council for Nautical Archaeology, the North American Society for Oceanic History, the International Commission of Maritime History, and the International Congress of Maritime Museums, to whose relevant programs the reader is introduced in the authors' bibliographic note and ample suggestions for further reading. A useful series of outline maps provides necessary orientation to a narrative that, though compact, relates the concurrent experience of maritime rivals in a well-measured style. For many students of naval history, oriented by Alfred Thayer Mahan and more recent surveys to the apparent centrality of Britain's naval experience, this panorama of more distant maritime conflict provides clear insight into the relationship between naval power and economic development, as notably illustrated in the rise and decline of Byzantine, Muslim, Latin, and Hanseatic maritime prosperity. Reflecting the findings of numerous recent monographs, the authors explore the relevance of venturesome trading communities, emergent industries, increasingly sophisticated financial institutions, and farsighted governmental policy in developing and maintaining maritime power during the Middle Ages.

In recounting the shifting fortunes of English monarchs during and after the Hundred Years' War, the authors effectively describe the harsh honing process that ultimately produced British sea power. Relative latecomers to piracy, commerce raiding, and amphibious operations, the British found a technique necessary to their survival, for centuries thereafter, in the convoy system, which had long been employed in the Mediterranean. Less obvious than the subsequent development of formal squadron and fleet tactics, the establishment of the convoy system under Edward III and its later development through parliamentary enactment must indeed be identified as a promising lacuna in our knowledge of this underrated era of maritime history. The arduous conditions of life and combat at sea, graphically suggested by contemporaries, also await further clarification from present and future research at the sites of medieval shipwrecks.

PHILIP K. LUNDEBERG
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MODERN EUROPE

NORMA E. EMERTON. *The Scientific Reinterpretation of Form*. (Cornell History of Science Series.) Ithaca: Cornell University Press. 1984. Pp. 318. $29.95.

Historians of early modern science have concentrated mainly on mechanics, astronomy, and biology. Norma E. Emerton turns to the much less studied domains of crystallography and mineralogy and uses the Aristotelian concept of form as a key to the sorts of explanation that were offered in these areas up to the early nineteenth century. She contends that, although the material and efficient causes dominated the explanation of these phenomena during the medieval period, form "made a remarkable comeback in the sixteenth and seventeenth centuries" caused mainly by "the increasing interest in crystals, coupled with an awareness of the

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shortcomings of mechanical explanations of crystallization" (p. 36). Crystals impressed natural philosophers because their geometrical shapes were an invariant characteristic of the kind of substance. Here, then, was an undeniable connection between shape and kind—one reminiscent of the ancient doctrine of substantial form. Thus, even as this doctrine was being repudiated in other areas of natural science, it took on a new life in crystallography and mineralogy and remained influential there until well into the nineteenth century. Emerton closes her narrative with its last great exponent, René-Just Hauy, who died in 1822.

She opens with a discussion of two topics in medieval natural philosophy that prepared the way for the later discussions of form. One was "mixtion," in which two or more substances combine to form a new substance. What happens to the original forms? Do they remain in some way in the new substance, or are they entirely replaced? The other topic concerned "minima," the smallest parts of a natural kind in which the form of the whole is preserved. This Aristotelian alternative to atomism was developed by Roger Bacon and other medieval philosophers. Oresme, for example, associated form with geometrical figure and thus suggested that the smallest part of a substance that could still count as an instance of that substance was distinguished by its form or shape. These debates raised issues about the role of form that would recur in discussions about the formation of crystals.

A number of metaphors governed these early discussions. Writers as different as Paracelsus and Gassendi spoke of "seeds"—the active principles needed to convert matter into specific kinds. Others, especially those influenced by Neoplatonism, invoked "spirit" as an active principle specific to a particular natural kind. Eventually "salt" became the primary formative principle, taking on the active transformative character of both seed and spirit, although some thinkers preferred "acids," and spoke of a "universal acid" as being responsible for the formation of all minerals.

The "mechanical" philosophers, on the other hand, were committed to explanations based on the sizes, shapes, and motions of constituent corpuscles. Thus, Robert Boyle noted that the power of salt to harden other bodies proceeded not "from any peculiar and inexplicable property" but "from the shape and motion of its corpuscles" alone (p. 146). Nevertheless, elsewhere he conceded that a "plastick principle" might be at work here but complained that its function had not been specified. In this context "form" seems to reduce to merely the shapes of the constituent particles.

Isaac Newton's notion of attraction, however, implied that the forces between the constituents of crystals, rather than their shapes, determined the form of the crystal. But this lead would not be followed until long after. Throughout the eighteenth century ideas such as that of a "primitive nucleus" from which mineral or crystal could grow recalled the older metaphors.

Emerton's book is a valuable resource for students of this subject. Her central contention, however, that the various approaches to the formation of crystals and minerals can be understood as "reinterpretations" of the older Aristotelian notion of form, is unconvincing. Although the word "form" continued (and continues) to be used and "seeds," "salts," and "acids" had an affinity with one strand, at least, of the older tradition, the identification of form with shape and the consequent explanation of crystal form by the shape of the constituent basic corpuscle seems more an abandonment than a reinterpretation of Aristotle's concept. Considerations of space led the author to ignore biology, but I wonder if this story can be traced without adverting to the domain that had inspired Aristotle's notion of form.

One complaint I feel bound to register concerns the odd organization of the narrative. For no obvious reason writers are often treated out of chronological sequence and are sometimes discussed more than once. The book would have greatly benefited from one last thorough revision. Nevertheless, the author has demonstrated that the eclipse of substantial form in those parts of science inspired by the new mechanics did not immediately eliminate this notion from natural philosophy.

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A. WEAR *et al.*, editors. *The Medical Renaissance of the Sixteenth Century*. New York: Cambridge University Press. 1985. Pp. xvi, 349. $59.50.

This book achieves what many collective volumes fail to accomplish: it offers a broad introduction to the best new scholarship in an important and rapidly changing field—in this case, the history of academic medicine in sixteenth-century Europe. Emphasizing Continental medical writing, primarily in France and Italy, it complements another fine collection from the same press, edited by Charles Webster, *Health, Medicine and Mortality in the Sixteenth Century* (1979), which focused on the demographic and social history of medicine, primarily in England. But, whereas Webster's volume surveyed a relatively new area of research, this book sets out to revise a more traditional one.

Historians have long recognized the sixteenth century as a period of profound change in medical doctrine, but they have tended to portray it in a selective and Whiggish manner. Most have focused on a small group of heroes (including Paracelsus,

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Pare, Vesalius, and their followers), isolated them from the rest of contemporary medical and intellectual culture, and presented them as the medical vanguard of the scientific revolution—hard-headed empiricists struggling against the dead weight of classical and medieval authority. The present volume offers an alternative picture. It assumes that the Renaissance in medicine formed part of the larger cultural movement of that name and that its coherence sprang from a rebirth of interest in classical culture rather than from a wholesale rejection of the past. Thus, this book transfers into medicine the insights that Charles Schmitt and others have established for philosophy: the overwhelming mass of Renaissance writing on the technical and theoretical disciplines taught in European universities did not deny the medieval tradition (despite contemporaries' occasional inflated claims to the contrary) but modified and transformed it in the light of renascent classical scholarship.

To describe this change over the entire field of academic medicine would be impossible, given the present embryonic state of research. Instead these twelve essays provide a series of case studies sketching the contours of the shift. Some discuss particular writers or groups of writers (Berengario da Carpi, Fernel, Fabrizio, the "Paris Hippocratics"); others treat particular historical or medical issues (for example, changes in editions of basic medical texts, controversies over the pulse, and writing on the occult causes of disease); still others study the various subdisciplines of medicine (including two fine articles by Richard Palmer and Vivian Nutton on pharmacy and academic surgery).

A remarkably coherent picture emerges of a well-established discipline in the throes of wide-ranging reform. This reform appeared with full force in Italy in the 1520s and spread rapidly to France and other European countries. Its spokesmen hoped to purge the medical canon of its textual impurities and its obsession with the fussy logical subtleties they associated with medieval Latin commentaries. At the same time most of the Paduan and Parisian Galenists and Hippocratics kept the traditional doctrinal and logical core of their discipline intact, while their classicism often became its own straitjacket, provoking a number of later sixteenth-century writers to call for a return to the Aristotelian logic and natural philosophy that had served medicine so well in earlier centuries (see the essays by Andrew Wear and Jerome Bylebyl).

Contemplating this complicated process of reform and reaction, we understand the impossibility of establishing clear lines between traditionalists and progressives, heroes and goats. Further, we begin to appreciate perhaps the most important revelation of this volume: the rich variety of sixteenth-century Latin medical writing and its diversity of genre, method, and style. Even the subdiscipline of anatomy embraced both the philosophically and physiologically oriented work of Fabrizio and Berengario's meticulous concern with visible structures, brilliantly analyzed by Roger French. Such contrasts show the simplemindedness of the old historical scholarship; they force us to appreciate the constant interplay between authority, reason, and experience in medical writing and to think about important distinctions between observation and experiment, pedagogy and research.

Like any collection of case studies, this volume contains many lacunae; except for Palmer's essay on pharmacy, the discussions of therapeutics leave much to be desired, and few of the contributions incorporate much of the new work on the history of universities or the impact of printing. The valuable articles by Palmer, Andrew Cunningham, and Ian Lonie on Venice, Padua, and Paris show how sorely we need detailed local studies of other medical centers, especially outside France and Italy. Nonetheless, this book sets a new standard for the intellectual history of sixteenth-century medicine and maps out the issues and approaches likely to be central to the subject for some time to come.

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MICHAEL MITTERAUER. *Ledige Mütter: Zur Geschichte illegitimer Geburten in Europa*. Munich: C. H. Beck. 1983. Pp. 173.

Michael Mitterauer has written an elegant and provocative study of illegitimate births. Despite its brief compass—the text is less than 115 pages in length—the book serves two purposes; it offers a synthesis of the recent social history of bastardy, and it reexamines Edward Shorter's thesis of a sexual revolution.

The concept and frequency of illegitimacy as a demographic phenomenon derive from the distance created by society between the biological capacity to bear children and the legitimate right to do so. Thus, by introducing the social distinction between maturity and marriage, Mitterauer arrives at his fundamental thesis. Illegitimacy is a historical function of the so-called European marriage pattern; it varies directly with age at marriage and practice of celibacy. Social and economic institutions (such as religious values, family organization, courtship rituals, and work patterns) that encourage later marriages—and so create larger groups of unmarried, sexually mature persons in a population—establish conditions for higher levels of illegitimate births.

The most spectacular surge of nonmarital fertility occurred in the late eighteenth and early nineteenth centuries and was associated by Shorter with a

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pervasive revolution in sexual behavior. Capitalism was thought to have created the profit-seeking individual whose desire for self-realization and self-gratification constituted a psychodynamic development, a wish to be free that extended to sexual behavior. Sexual freedom, according to Shorter, encouraged illegitimacy.

By rehearsing the sexual revolution, Mitterauer intends no gratuitous criticism of a theory already rejected by the vast majority of scholars. Abandoning Shorter's psychohistory in favor of social and economic explanations, he retains the basic notion but assigns different causes. Early industrialization, in which production was organized around the workshop and was highly labor intensive, created preconditions for the great surge in bastardy. In town and country appeared a mobile work force of servants, journeymen, and laborers who were forced to remain single by the difficulty of obtaining the basic means of production and independence—a household. Unmarried, undisciplined, and unstable, this early proletariat formed the population most subject to illegitimacy.

Mitterauer relies heavily on recent theories of proto-industrialization and proletarianization, which display an unfortunate tendency to oversimplify by asserting a single pattern of development as the basis of industrialization. He specifically rejects monocausal explanations of illegitimacy but commits the same error by insisting on the fundamental importance of delayed marriage because of lack of financial means. Although highly suggestive, such a theory dismisses other factors out of hand. In the only original research offered, the author examines reports on the moral behavior of the laity made by Austrian bishops to Emperor Francis in the early nineteenth century. Among the half-dozen explanations for high levels of illegitimacy identified by Mitterauer as typical in these documents, only one, large numbers of migratory workers, relates directly to his proto-industrial theory. Such a discrepancy suggests the potential weakness of an argument based essentially on a single factor, age at marriage.

Ultimately, Mitterauer recognizes that illegitimacy was closely tied to regional, social, and economic conditions that require individual study. This book will certainly inspire that research and much valuable debate.

THOMAS MAX SAFLEY
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MARY BETH ROSE, editor. *Women in the Middle Ages and the Renaissance: Literary and Historical Perspectives.* Syracuse, N.Y.: Syracuse University Press. 1986. Pp. xxviii, 288. Cloth $29.95, paper $12.50.

These essays are an interesting addition to the growing number of studies about women in the Middle Ages and the Renaissance. They are the tangible result of a conference on changing perspectives on women in the Renaissance held in Chicago in 1983. The book's title, however, does not make clear that the vast bulk of the work is about Renaissance women, and so those primarily interested in the Middle Ages are bound to be disappointed in it. But the essays are worth reading as a contribution toward a greater understanding of women and attitudes to women in an earlier period of our history. All but one of the eleven contributors are women.

The first six essays, commencing with "Women's Defense of their Public Role" by Merry E. Wiesener, deal with the impact of religious, economic, social, and cultural norms on women's lives from the thirteenth to the seventeenth centuries. Exploring the idea of freedom, Wiesener agues that for women it meant the ability to participate in public life. She surveys women's contracting public role, which was opposed by the most able women in every generation. Often by dint of claiming to be "special cases," women continued to express a desire for public service in trades, medicine, and learning. Their arguments were often, ironically, antifeminist in that their need for action caused them to set themselves apart from the general run of their sex.

Jane Tibbetts Schulenberg's essay entitled "The Heroics of Virginity" suggests that the epigram "cutting off your nose to spite your face" originated in the cases of women committing self-mutilation to defend their virginity against rape in the early Middle Ages. She looks first at patristic writings, then papal, canonical, and conciliar statements on the subject of virginity and shows that such writings established virginity as a perfect way of life for women in religion. The need for a virgin "to remain both spiritually and physically intact" if she was to merit her virgin's crown in heaven gave rise to a complementary response of fear and guilt when faced with the possibility of rape at the hands of violent groups such as barbarians, Saracens, or Vikings. The emphasis of the church's teaching meant that female religious had a lot more to lose in such a situation than their male counterparts. The four examples cited of self-mutilation in the face of certain assault provide considerable food for thought, although Schulenburg admits that some critics dismiss them as legends.

Madelon Sprengnether's essay on *Coriolanus* takes a psychoanalytical approach to the Shakespearian tragic hero's relationship with the women in his life. Carole Lewis's essay, "John Foxe and the Responsibilities of Queenship," teases out Foxe's approach to a mighty ruler (whom he respected) who was also a woman (whom he regarded as a frail vessel). Leah

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Marcus's essay "Shakespeare's Comic Heroines, Elizabeth I and the Political Uses of Androgyny," analyzes the attempt of the Virgin Queen to unite both her princely powers and her feminine nature in her public utterances.

The last five essays deal with female writers and attempt to define the particular problems they faced in breaking into literary culture dominated by men. The struggle of Margery Kempe, wife of a burgess of Lynn, to pursue her spiritual calling to be accepted as a bride of God is analyzed in Janet Mueller's essay, "Autobiography of a New 'Creatur': Spirituality, Selfhood, and Authorship in *The Book of Margery Kempe*." Mueller shows that Margery was well aware of the incongruous nature of the vocation she followed—to be a bride of God, entitled to wear white clothing and gold ring—and that that awareness can be detailed in the writing of her book. Her life is seen as a kind of quest, first to realize her goal as God's bride and then to find a role that would fully express that loving union for the rest of her life.

The editor, Mary Beth Rose, provides the final essay, "Gender, Genre, and History: Seventeenth-Century English Women and the Art of Autobiography." It illustrates excellently the way women reconciled the demands of both private and public lives in their work. The chaos of the civil war in England had a liberating effect on the four women she studies—Margaret Cavendish, duchess of Newcastle (1625–74), Anne, Lady Halkett (1623–99), Ann, Lady Fanshawe (1625–80), and Alice Thornton (1626/7–1706/7). Their secular autobiographies are among the earliest examples of the kind. This female creativity was engendered by a social upheaval and found expression in a form that was already in existence but that these four women helped to direct toward an interest in the author's own identity and experience. These women thought it worthwhile to "attempt to create themselves in literary form" by setting a value on their own secular experiences (p. 274).

As well as providing the last essay, the editor also contributes an introduction, which sets the whole collection in the context of the attempts of (mainly) women scholars of a variety of disciplines to advance the study of female identity and experience. An index and some illustrations have also been provided.

ANGELA M. LUCAS
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F. A. WOLF. *Prolegomena to Homer (1795)*. Translated by ANTHONY GRAFTON *et al.* Princeton: Princeton University Press. 1985. Pp. xiv, 265. $42.00.

Prussian Latin, which was devised to make Prussian science available to an international audience, became extinct as a spoken language around 1910 because the sheer excellence of Prussian science caused this audience to learn the language Latin was meant to replace. Friedrich August Wolf's *Prolegomena ad Homerum* (1795), a brilliant and influential book, has probably been read through by less than fifty people since 1918. *Laudatur et alget*. Three young Princeton scholars, instead of producing yet another translation of Virgil's *Aeneid*, have restored *Prolegomena* to the English world. A true translation is metempsychosis, and, as I read the book, I began to think that Wolf wrote in an occasionally ponderous English. That I have detected no error does not mean that none exists, but I should be surprised if much is wildly wrong. Two sets of notes adorn the text, Wolf's, with modernized references to ancient sources added in brackets, and the translators', which elucidate references in Wolf elusive for modern readers.

In his introduction Anthony Grafton seeks to place the book into its historical context, Homeric studies in 1795. In my opinion he rightly stresses two matters. He elaborates Ulrich von Wilamowitz-Moellendorff's judgment that *Prolegomena* was a response to J. P. G. d'A. de Villoisin's edition (1788) of the Venetian scholia to Homer and that its abiding value resides in its contribution to the history of textual transmission rather than in any alleged discovery of the Homeric question. He stresses also Wolf's methodological indebtedness to the Old Testament scholar J. G. Eichhorn (1752–1827), particularly in regarding the Venetian scholia as a "Greek Masorah" and in seeing the crucial importance of the history of the text before the standard Alexandrine edition. I should add that twice later Old Testament analysis determined the direction of Homeric studies. The heresy trial of Robinson Smith in Aberdeen (1877–81), resulting from his post-Mosaic dating of Deuteronomy on analytical grounds, instigated English Homeric analysis as protest against ecclesiastical orthodoxy. A methodology allowed Homer cannot be denied Moses. In 1884 Wilamowitz-Moellendorff, the most influential Hellenist of modern times, explicitly admitted his indebtedness to Julius Wellhausen's research on the Pentateuch.

Grafton has been sharply criticized as a historian of classical scholarship who has never proven himself as a classical scholar. His limitations are obvious here. There are naive superlatives (pp. 4 n. 6, 6). He attributes the suggestion of a Pisistratean recension to Cicero and Josephus with no idea that their source is Dieuchidas of Megara (p. 5). He omits fundamental secondary sources, such as G. Finsler's *Homer in der Neuzeit* (1912) and especially Wilamowitz-Moellendorff's "Homerische Untersuchun-

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gen," *Philologische Untersuchungen* 7 (1884): 381–419. He lacks the judgment to know that R. J. White's life of Bentley is derived incompetently from J. H. Monk and cites it as authoritative (p. 6 n. 12). He cites English translations of Vico, Schelling, Schleiermacher, and others without reference to the original text and regularly cites an author as cited in a secondary work rather than from the primary source (for example, Goethe, p. 33 n. 94; Voss, p. 26 n. 65). This makes the reader do the author's job. That a professor of history at Princeton University in 1985 confuses Napoleon's defeat of Prussia at Jena-Auerstadt in 1806 with Prussia's defeat of Napoleon at Leipzig in 1813 is sobering (p. 4). Hermann Muchau's German translation of 1908 is unknown.

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MARIA WAWRYKOWA. *"Für eure und unsere Freiheit": Studentenschaft und junge Intelligenz in Ost- und Mitteleuropa in der erstern Hälfte des 19. Jahrhunderts.* (Schriften der Mainzer Philosophischen Fakultätsgesellschaft, number 10.) Stuttgart: Franz Steiner. 1985. Pp. 183. DM 48.

Over the past twenty-odd years Maria Wawrykowa has published several excellent studies of the Enlightenment in Germany and Poland, particularly on the development of schools and of the intelligentsia in the early nineteenth century. Here these topics are presented in a comparative framework that includes Russia as well as Germany and Poland. A brief introduction poses the main questions to be addressed: What was the intelligentsia, where did it come from, and what were its relations to the rest of society? A chapter on the Enlightenment describes the "world of the fathers" and is followed by one on romanticism, the "world of the sons." A chapter on the 1820s precedes a much briefer one on the secret societies of that period. A short chapter on the Polish revolt of 1830 is followed by one that describes at length the young intelligentsia's attempts to develop international cooperation in the 1830s and 1840s. A final chapter describes the development of the Russian intelligentsia in the 1830s and 1840s. A brief summary concludes the book.

Wawrykowa finds great variety in the three intelligentsias studied. They came from different social classes, they experienced quite different occupational and career histories, and their composition changed over time, sometimes quite rapidly, in response to the different situations in the three countries. Nonetheless, the three groups sympathized with one another and made remarkable efforts to collaborate. Despite differences among them, all focused on much the same problem: the crisis of belief that followed the collapse of faith in enlightened rationalism. All rejected the enlightened faith of the "fathers" and sought to find a "new way" out of the situation left by them.

This book has many virtues. It provides a very well informed discussion of the development of various groups of the intelligentsia, often, but not always, composed of students or recent students, and is particularly strong in making clear the role of politics, that is events, and the importance of the study of history in forming the ideology of the intelligentsias. Particularly on Polish groups and on Polish efforts at collaboration with others, Wawrykowa offers both information and analysis not easily gained elsewhere. About 90 percent of the more than four hundred notes are biographical identifications of figures mentioned in the text, giving a line or two for well-known figures and much more for the less known.

In previous publications Wawrykowa has well documented much of the analysis briefly presented in this book. Since many of her publications are in Polish and not widely available outside Poland, readers of the present study might have been served better by discussion of sources (for example, for the figures on Polish students [p. 79]) rather than brief biographical notes (for example, for Tsar Alexander I, [p. 85]). The text also seems unbalanced. The introduction points out that German aspects of the study are treated briefly because German students (this work originated as a lecture series at Mainz University) are likely to need more detail on the Polish and Russian aspects. Unfortunately, the schools, particularly the universities, are also discussed briefly, especially in comparison with the attention given general intellectual currents. Thus, the universities' significance is understated, if not omitted. For example, the discussion of the Society of Philomats at Vilna University suggests the similarity with German student organizations but seems to miss the local factors that are particularly important in the case of the Philomats. Finally, although completely commanding Polish, German, and older Russian work, Wawrykowa pays little attention to the important contributions made by Soviet, American, and French scholars. For example, Daniel Beauvois's history of Vilna University, 1803–32, is too important to the subject not to be cited, at least. Wawrykowa has presented, clearly and thoroughly, a discussion of the issues that contributed to the developing Polish intelligentsia in the first forty-odd years of the nineteenth century and has illuminated her discussion with extended comparisons with contemporary developments in Germany and Russia. That is a worthwhile achievement, if not quite what the book's subtitle seems to promise.

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DAVID PIKE. *Lukács and Brecht.* Chapel Hill: University of North Carolina Press. 1985. Pp. xvi, 337. $29.95.

David Pike's book, a spin-off from his monumental *German Writers in Soviet Exile, 1933–1945* (1982), is narrower in scope than its title suggests. Pike focuses on Soviet political and cultural influences on György Lukäcs from 1928 to Stalin's death in 1953 and on Bertolt Brecht from 1933 to his move to the German Democratic Republic in 1948. The study deals separately with Lukács and Brecht but links the two in what Pike calls their mutual "betrayal of reason."

Pike devotes two-thirds of the book to Lukács, the most eminent Marxist theorist outside the Soviet Union. In the introductory chapters the author asserts that Lukács betrayed reason with this hypertrophied rationality, Procrustean intellectuality, "Double-think," and zest for casuistry. Ensuing chapters that delineate Lukács's life and works provide more detailed documentation on these points. Pike reserves special scorn for Lukács's moral complicity in the death of six million people in the Soviet labor camps. In my opinion, Pike's concluding assessment that Lukács "managed to secure scarcely more for himself than a fleeting sort of fame" (p. 191) will not stand the test of time. Lukács's last work, *The Ontology of Social Existence* (1978), the first Marxist ontology since Marx, is, at the very least, a seminal work.

Pike's investigation is less innovative in the three chapters on Brecht, in which he should have recognized John Fuegi's and Marjorie Hoover's pioneering studies. Pike, however, is the first critic to give a comprehensive account of Brecht's reception in the Soviet Union during the 1930s, although he barely mentions Aleksandr Tairov's brilliant staging of *The Threepenny Opera*, probably because it does not support his theory of the "betrayal of reason." Instead, Pike devotes much space to the Brecht circle's abortive efforts to stage *Die Rundköpfe und die Spitzköpfe* and *Die Heilige Johanna der Schlachthöfe* in Moscow. He rightly rejects Bernard Reich's view that political pressures caused difficulties in staging *Die Heilige Johanna*. As I learned from authoritative sources in Moscow, the play actually was not produced because of insurmountable casting problems. More generally, Pike does not set forth convincing arguments for Brecht's tantalizingly vague attitude toward the Soviet Union. In addition, the author fails to convince me that the "betrayal of reason" played a major part in shaping Brecht's life and works.

Despite its shortcomings, Pike's book contributes significantly to Germanic and Slavic studies as well as to social history. All future research in this field must take this meticulous and judicious analysis into account. It is a pity that, by publishing his two books, Pike has unavoidably jeopardized his chances to returning to his Soviet archival sources. A study of related areas still begs to be researched.

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PHILIP CORRIGAN and DEREK SAYER. *The Great Arch: English State Formation as Cultural Revolution.* Foreword by G. E. AYLMER. New York: Basil Blackwell. 1985. Pp. viii, 268. Cloth $34.95, paper $15.95.

The book is a curious compound of historical insight, dogmatic assertion, bibliographical annotation, and political polemic. Philip Corrigan and Derek Sayer are two youthful sociologists who draw on the works of a large number of modern historians—as well as on Karl Marx, Max Weber, and Emile Durkheim—to trace the evolution of the concept of the state in England from the Norman Conquest to the end of the nineteenth century. That concept came to define for the English (and others) every significant aspect not only of their political but also of their cultural world. The state was, therefore, not merely a "super structure" but rather the entity that, with its associated institutions and images, proved "fundamental to the construction of English capitalist civilization" (p. 192).

Corrigan and Sayer devote a chapter to each of six chronological building blocks of "the great arch." The best-written chapters are those on the Middle Ages and the revolution of the 1530s. Many of the points they make about England's medieval "exceptionalism" are unexceptionable, and Geoffrey Elton should take no offense at the manner in which his works are summarized. Admittedly, many truisms of English constitutional history are proclaimed with a breathless air of discovery; for example, the reign of Henry VIII resulted in a strengthening of the state at the expense of a united Christendom on the one hand and the power of mighty barons on the other. Rather than postulating a single triumphant "bourgeois revolution" in English history—whether in the 1640s or in the 1830s—the authors stress the gradual "centuries-long embourgeoisment of England's dominant classes (and proletarianization of the ruled, the two being inseparable)" (p. 11). They solve the historiographical problem of the survival of landed aristocrats and squires by redefining both as "vigorous agrarian capitalists" (p. 88). Each successive era—whether government by unpaid justices of the peace in the eighteenth century or by centrally paid factory inspectors and products of the civil service examination system in the nineteenth—turns out to have had a single significant consequence: the advancement of a capitalism by definition exploitative, patriarchal, and injurious. Drawing largely on E. P. Thompson, the authors summarize the first decades of the nineteenth cen-

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tury as "the years of what we can without exaggeration call the English terror, when the working class was hammered and machined into acceptable relations" (p. 115). Although the lengthy chapter on the later nineteenth century is marred by an almost unreadable admixture of theorizing, quotation, summary, and historiographical debate (carried on in parentheses as well as endnotes), the book provides many fascinating historical nuggets as well as a suggestive underlying thesis.

The difficulties posed by the book are not, however, solely stylistic. Although Corrigan and Sayer appropriately examine unarticulated assumptions about the neutrality or universality of the claims of "the state," they indiscriminately employ concepts such as "bougeois civilization" and "the working class"—oblivious to chronology and to the diversity of economic assumptions, arrangements, and occupations. The worker is ever portrayed as the victim and never as the beneficiary of the private enterprise of others in housing, health, or comfort. Certain that "it was specifically in England that modern capitalism was so spectacularly to triumph" (p. 85), the authors ignore arguments such as those of Martin Wiener that capitalist values triumphed there only in part and that the malaise of recent decades is the result of too little reliance on the market system rather than too much. Corrigan and Sayer rail passionately against the centuries-old subordination of women (characteristic apparently only of bourgeois societies) yet utterly ignore the legal powers of unmarried and widowed female property owners and forget the significance they have themselves attributed to reigning queens such as Elizabeth I. They reserve their sharpest taunts, moreover, for the first female prime minister. Although they concede that many twentieth-century socialist experiments have proved bloody, they continue to judge modern English society by a visionary, romantic socialist standard untrammeled by experience and to look ahead to a new "cultural revolution" that will truly "emancipate" and "liberate" the oppressed. Like faithful workhorses wearing blinders, Corrigan and Sayer see some portions of the historical landscape with illuminating clarity and others not at all.

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MARGARET PATTERSON HANNAY, editor. *Silent But for the Word: Tudor Women as Patrons, Translators, and Writers of Religious Works*. Kent, Ohio: Kent State University Press. 1985. Pp. 304. $27.50.

Before prospective readers decide, with some justification, that seldom has so much been written about so little, they must remember that this volume is in fact a study in silences, in what is not there but under different circumstances could have been. One of the paradoxes posed by Renaissance England is that, although more women than ever before were being given what amounted to advanced education and could presumably have shared accordingly in the creative culture, relatively few did, and even they contributed relatively little. At best, a few turned their talents toward translation, and, what was still more limiting, almost exclusively toward translation of religious works. The fault was not theirs, of course, and these essays, in part, attempt to explain why. As is argued here, justifiably, if hardly surprisingly, these humanistically trained women were inhibited from original creative effort by a traditionally male-dominated society. Their choice of subjects was further limited by the strongly Christian character of English humanism and by the pressures indirectly exerted by confessional controversy. Thus, as authors, women could eke out only a precarious, sometimes even an anonymous, existence on the fringes of literary life or, perhaps more effectively in the long run, assist in the indispensable process of patronage. These rare spirits were, at times, however, able to inject personal views into these endeavors.

The essays in this volume, edited by Margaret Patterson Hannay, document with admirable thoroughness the plight of the educated and potentially creative woman in Renaissance England. They also insightfully trace the conventions of the male-dominated society and the attitude that took for granted that a woman's place was naturally in the home, that virtue was her main concern, and that silence was for her a primary desideratum. Whether all this amounted to a male conspiracy, as is here and there implied at the very least, is quite another matter. The notion, suggested in one essay, that the men of the period felt threatened in their very sexuality by the occasional advent of a learned and articulate woman is unconvincing. Surely Juan Luis Vives would have been surprised to find his program for the education of women considered the repressive document it can only too readily be made to appear to us. In fact, the contributors have not always been alert to the danger of applying present values to the analysis of a past and very different society. Yet, one cannot help but be caught up in the spirit of righteous indignation that permeates these pages. One cannot help but ask with the authors what might have been the contribution to letters and scholarship had not Margaret Roper or Mary Sidney been kept from expressing themselves freely by a society unsympathetic to the creative effort of women? As the author of a concluding theoretical chapter argues, a society must be understood in terms not only of its discourse but of its silences.

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Since the women dealt with in these essays wrote relatively little and since their silence "but for the word" must be explained by the circumstances of their lives, most of the contributions tend toward biography rather than criticism. The collection contains some very good biographical research and writing and is well worth reading by anyone interested in the culture of Renaissance England.

ARTHUR B. FERGUSON
Duke University

CHRISTOPHER HILL. *The Collected Essays of Christopher Hill.* Volume 2, *Religion and Politics in Seventeenth-Century England.* Amherst: University of Massachusetts Press. 1986. Pp. xi, 356. $25.00.

Retirement evidently agrees with Christopher Hill, for this second volume of his current series of collected essays is not the tired scourings of a neglected desk but a vigorous revision of lectures, reviews, and essays that, although written over the past twenty years, presents a remarkably coherent picture of Hill's current vision of the role religious ideas played in seventeenth-century English politics. For, in these essays, Hill is primarily concerned with ideas and ideology, with what people thought and meant, rather than with ecclesiastical institutions, with the church as such. As he remarks with pardonable irony in the last essay, "even Marxists have been known to speak of Puritanism as the ideology of the English revolution. God still has a role in seventeenth-century politics" (p. 321). A few of these fifteen essays are slight—that on John Reeve, Laurence Clarkson, and Lodowick Muggleton, for example, is little more than an addendum to his recent work on the Muggletonians—but others are major pieces, including the reworked magisterial essay on Gerrard Winstanley's religion. All reflect Hill's perception that "environment is more important than heredity in the evolution of ideas" (p. 4): ideas count, but, although ideas have their own logic, that logic will be pursued only if it is useful to someone or some group.

The tone of these essays is notably unpolemical, but he does gently question Patrick Collinson's recent contention that scholarly and conscientious bishops of an Ambrosian independence presided over a peaceful Protestant church in late Elizabethan and Jacobean England until that house of many mansions was gutted by the ruthless reforms of William Laud. Hill's critique of this happy view is telling, but, in another essay, he quotes John Owen, in a mood of Collinsonian nostalgia, insisting that he and the other dissenters "do sacredly adhere unto . . . the doctrine of the Church of England . . . declared in the authenticated writings of all the learned prelates and others for sixty years after the Reformation" (p. 302). The other major theme is once again radical religious ideas and radical politics. Three important, interconnected essays deal with sin and society, antinomianism in seventeenth-century England, and Tobias Crisp, (1600–43), a conscientious Cambridge Calvinist who, to avoid the covenant of works he saw being smuggled back into Reformed Protestantism by the covenant theologians, retreated cautiously into antinomianism. Although each reader will have favorites, a final gem must be mentioned: the essay on occasional conformity, which suggests that we see the practice less as a device of hypocritical accommodation than as an attempt "to preserve links with the state church, for social as well as theological reasons" (p. 308).

These essays are vintage Hill, suggestive and illuminating by turns, appreciative of nuances of thought as well as of politics. No other historian has done more to make us take seriously the religious thought of the period.

PAUL SEAVER
Stanford University

CLYVE JONES, editor. *Party and Management in Parliament, 1660–1784.* New York: St. Martin's or Leicester University Press, Leicester, England. 1984. Pp. xvi, 205. $22.50.

This collection of six articles by various historians deals with several loosely connected aspects of British parliamentary politics. Clyve Jones is the editor of the book.

Richard Davis's theme of "Presbyterian" opposition in Charles II's House of Lords is not limited to Presbyterians, Protestant dissenters, or even Protestants, and his deliberate adoption of lax contemporary terminology could spoil an otherwise useful account by placing too much emphasis on a Nonconformist faction that was probably declining. David Hayton provides a lucid discussion of the relationship between the "country" and the party system from 1689 to circa 1720. Hayton concludes that "'Court and Country' ceased to represent a standing political division," and advises that we throw away Robert Walcot's compass and regard court-country conflicts as "superstructural" to the two-party system of Whigs and Tories. A sensible account of the constitutional case of Ashby versus White, 1701–04, is offered by Eveline Cruickshanks, who describes a Whig attempt to thwart the Tory majority in the Commons and to break the Tory interest in the borough of Aylesbury. W. A. Speck gives a lively description of the Commons' disgraceful handling of electoral disputes (even by contemporary standards) from 1702 to 1742. By Walpole's time the Tories had virtually ceased to petition against unfair electoral practices—it was a waste of

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effort and expense. But the situation was not irremediable, and Walpole's loss of control of the Committee of Elections after a closely fought general election in 1741 was his undoing. Jones analyzes the struggles of Robert Harley to maintain a governmental majority in the House of Lords from 1711 to 1714 and shows how Harley's "management" anticipated Walpole's later and more successful efforts in the same chamber. This chapter has interesting detail on the events leading up to Harley's famous creation of twelve Tory peers to pass the Peace of Utrecht in 1712. In the only contribution to go beyond 1742, Paul Kelly gives a well-researched description of the rise of the idea that constituents' instructions to their representatives should be mandatory. He concludes that before the London area's instructions to its representatives to press for parliamentary reform in 1769 the direct precedents are dubious, although embryonic examples abound.

All the articles make worthwhile contributions, and Davis, Hayton, and Jones supply useful division or other lists. The print is small, and lines are not justified on the right.

BRIAN HILL
University of East Anglia

M. M. GOLDSMITH. *Private Vices, Public Benefits: Bernard Mandeville's Social and Political Thought.* New York: Cambridge University Press. 1985. Pp. ix, 183.

What started out twenty years ago for M. M. Goldsmith as a brief excursion into the life and writing of Bernard Mandeville has resulted in a probing and well-informed monograph. Goldsmith's evidence is so abundant and convincing, his knowledge of the literature on Mandeville so intimate, that his image of Mandeville as the most sinister English satirist of the eighteenth-century is difficult to challenge.

In addition to its erudition, a good deal of the strength and the attraction of this study is founded in the author's ability to execute what is often promised but seldom delivered. Mandeville's menacing ideas are placed with great precision in the context of the vast pamphlet literature of early eighteenth-century politics. In the context of these pamphlets Goldsmith's Mandeville is distinguished by his steadfast refusal to accept the civic humanism of the Machiavellian-Harringtonian tradition of public virtue as well as conventional Christian morality with its insistence on private virtue. To Mandeville the Augustan moralists, with their incessant talk of the danger of public corrution and private vice, were tiresome because they were unrealistic. Mandeville's views on pride and virtue first emerged in *The Female Tatler* (1709), an attack on Richard Steele's portrait of the priggish Isaac Bickerstaff. They were further developed in *The Fable of the Bees* (1714), refined in the second part of that famous work in 1729, and given further consideration in Mandeville's last major work, *An Enquiry Into the Origin of Honour* (1732).

These works, which span Mandeville's productive years, are united by his grim assessment of human nature as well as by his sardonic treatment of both Whig moralists and Tory divines. Goldsmith situates Mandeville as a Whig, although hardly of the self-congratulatory and moralizing sort that subscribed to *The Tatler*. Mandeville was a Whig, I suspect, much in the same skeptical way his critic David Hume was—*faute de mieux*. Mandeville's theories on the origin of society and civic responsibility reject more than the pious platitudes of most contemporary moralists; they reject all of Christian teaching, at least as Goldsmith sees it, and offer instead a Hobbesian view of human nature, held in check by fear of disapprobation or by the necessity of survival in any communal sense. Tough stuff, and it did not go over well. Even those as disenchanted in their view of the origin and development of society as Hume refused to embrace the lugubrious Mandeville.

Goldsmith convincingly argues that Mandeville could not be considered a satirist in the manner of Swift or Pope, although he shared their contempt for pretentious preachers and politicians. Mandeville cannot believe the very premises of Christian or even common-sense morality, which these so-called Tory satirists claimed were being subverted by Whig ministries in the 1720s and 1730s. The foundations of civic humanism and Christian morality were convenient fictions as far as Mandeville was concerned. Goldsmith calls *The Fable of the Bees* a mock satire in which the "norms" that Swift decried as having been violated by the politicians, dunces, and mad scientists are shown to be fraudulent themselves. Mandeville's view of social development is as uncomplicated as it is disturbing. People come together and stay together out of necessity or because they fear being caught at following their natively selfish proclivities. Clever politicians can manipulate social behavior by exploiting this sorry state of human nature.

Goldsmith examines every facet of Mandeville's thought, even the most often overlooked area of economic theory, and he has no trouble making Mandeville into the type of capitalist governed by greed and acquisitiveness that Marx could easily denounce. The result of Goldsmith's patient labors is an outstanding study of ideas in context and a fine addition to the Cambridge University Press series of that title.

VICTOR G. WEXLER
University of Maryland, Baltimore County

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PETER JUPP. *Lord Grenville, 1759–1834*. New York: Clarendon Press of Oxford University Press. 1985. Pp. x, 506. $45.00.

This masterful study of William Wyndham Grenville is the first full-length biography of a central figure in British politics from 1782 to 1817. Peter Jupp accounts for this long-existing lacuna by observing that Lord Grenville's aloof and haughty personality discouraged biographers, who were attracted to more charismatic worthies of the period. Biographers may also have been repelled by the clannish and grasping Grenville family faction. A more compelling reason, however, is that Grenville, despite his importance, was a statesman of second rank who never enjoyed a period of "unrivalled political authority" or followed a "prolonged course of consistent political purpose" (p. vi)—subjects tending to attract political biographers. Moreover, the difficulty of access to the large collection of Grenville's public and private papers was an obstacle.

The Grenville collection, fortunately now in the British Library, is Jupp's principal source, but no other relevant source, manuscript or printed, has been overlooked. The result of the author's enormous labor is a work of mature scholarship that reflects the winnowing and thinking of nearly two decades. Cast in the traditional "life and times" format, this well-written biography hardly succeeds in bringing the subject to life, but "Bogey's" austerity would make that virtually impossible. Jupp, however, brilliantly succeeds in explaining Grenville's motivation at each of the critical points in his long career and gives a thorough account of the political background. Students of British history of the momentous thirty-five years spanned by Grenville's active career will find this biography a mine of information and sound interpretation.

Grenville's career was built on outstanding assets of intellect, learning, and family connection. Elected to Commons in 1782 from a family borough, he went the same year to Ireland as chief secretary under his older brother, the lord lieutenant. The next year he was made paymaster general when his cousin William Pitt became prime minister. For the first half of his career he ably served Pitt in various capacities, such as member and speaker of Commons, member and officer of the Boards of Control and Trade, and as a member of special diplomatic missions. According to Jupp, Grenville was virtually Pitt's vice-chancellor in these early years. For these services he was amply rewarded with the prizes he most coveted: cabinet office as home secretary in 1789, a peerage in 1790, and a lucrative sinecure in 1791.

As a member of the House of Lords, Grenville became Pitt's mainstay in directing government business in that body. But Grenville attained his greatest influence as foreign secretary from 1791 to 1801. The defeat of France in Europe was the main objective of his diplomacy and strategy. Jupp is at his best in explaining the intricacies of Grenville's contribution to the formulation and execution of policy, both foreign and domestic, during the wars of the French revolution. As a wartime measure, Grenville warmly supported the union with Ireland based on Catholic emancipation that contributed to the downfall of Pitt's government.

When Grenville left the Foreign Office, he began the second half of his career. In disagreement with Pitt over policy and associates, Grenville gravitated toward the opposition Whigs and became prime minister in the ministry of "All the Talents" in 1806. Because of the king's continuing intransigence on the Catholic question, the government of the Talents fell in 1807, and Grenville assumed leadership of the opposition until his retirement from politics in 1817.

Jupp's final assessment of Grenville is a model of compression and wisdom. Astutely ignoring the specious use of Whig and Tory labels for the contending political factions, Jupp characterizes Grenville not as a "stern and unbending Tory" but as a pragmatic politician in the Whig tradition who manifested both conservative and progressive tendencies. Generally a conservative in constitutional matters, he resisted on occasion the monarch and the prevailing government and opinion. Until the final defeat of France he resolutely supported the war effort to restore the equilibrium that was necessary to British security. Grenville, indeed, should receive much of the credit for devising the strategies leading to victory and enduring peace. As a conservative aristocat, he naturally upheld law and the existing social order during the tumultuous years of war and social unrest in a changing society. Yet his progressive Whiggery stemmed from enlightened and humane views. An early disciple of Adam Smith, he was long a champion of free trade, and his enlightenment was further reflected in his support of Catholic emancipation. His humanitarianism triumphed in the successful effort to end the slave trade during his brief tenure as prime minister. Jupp correctly concludes that the history of British government and politics from 1782 to 1830 could not be written without reference to Lord Grenville. One might add that without reference to Jupp's splendid biography much of that history would remain obscure.

MALCOLM LESTER
Davidson College

JOHN V. PICKSTONE. *Medicine and Industrial Society: A History of Hospital Development in Manchester and Its*

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Region, 1752–1946. Dover, N.H.: Manchester University Press. 1985. Pp. xi, 369. $50.00.

Many hospital histories have been written, shelves of them in fact, but few merit scholarly attention. Among the older histories of British hospitals Archibald Clark-Kennedy's two-volume history of the London Hospital (1962) and Brian Able-Smith's study of modern British hospitals (1964) are happy exceptions. I have been reminded in preparing this review how valuable those two works still are. Recently, hospitals have received fresh attention from social historians. Hospitals provide a convenient means of assessing the mutual influences of professional, social, and scientific or technological trends. They serve as institutional microcosms of complex changes. The history of groups of hospitals rather than of individual institutions is finally being written. Morris Vogel's recent one-volume history of Boston hospitals proves that a suggestive, incisive, and concise history of a community's hospitals is possible.

John V. Pickstone's volume is the first of a trilogy planned on the history of the hospitals in the area now administered by the National Health Service's Manchester Regional Hospital Board. The two subsequent volumes will deal, in turn, with the history of the region's mental hospitals and with nonmental hospitals under the National Health Service (1948–74). The project began in the desire of the hospital board to record its work, and the board provided part of the funding for the project. Its origins partially explain the work's approach. Although scientific and technological forces are given some attention and the attitudes of doctors or patients are occasionally mentioned, this is primarily an institutional and administrative history—hospital history written from the perspective of the board room. We are given very thorough and detailed accounts of the founding, expansion, moving, and merging of hospitals. Changes in sources of funding and of institutional use are also carefully described.

The text reflects careful research in the archives and in published sources. The author is cognizant of recent work in social history and in the history of science and medicine. Pickstone is no apologist and deals critically with his subjects.

Yet his text is dense. At times he strains the reader's endurance with the weight of raw facts. Aids to digestion, such as quantitative analysis or the use of tables, diagrams, or graphs, are few. The book includes a map of the Manchester hospital region showing county boundaries and the location of towns, but I would have appreciated the inclusion of a simplified map of Manchester showing the place names cited in the text.

The challenge of regional history is to endow local events with more universal significance. Vogel's book, which is admittedly more limited in chronological and geographical scope than this one, does this admirably. Pickstone faced a more daunting task and has not been as successful in this regard. His text is more unified and of more general interest in the third and final section, which deals with the construction of the National Health Service. But here the text's focus changes from local events to governmental plans for the new health service. In earlier sections, occasional generalizations on social, scientific, or educational trends are obscured by the mass of detail.

This failure fully to subordinate fact to interpretation is particularly disappointing because the book deals with an important subject, and so much research has gone into its preparation. The book remains a valuable source of information on the specific institutions described.

JOHN M. EYLER
University of Minnesota

NOEL W. THOMPSON. *The People's Science: The Popular Political Economy of Exploitation and Crisis, 1816–34*. New York: Cambridge University Press. 1984. Pp. viii, 252. $39.50.

Noel W. Thompson's examination of the ideas of the socialist political economists who flourished in Britain for about a decade after 1825 is a valuable addition to the literature. The book would have been more useful had Thompson not analyzed those ideas in relative isolation from their political and social surroundings.

A shifting set of economic ideas was expressed in the radical newspapers, pamphlets, and books that provide Thompson's sources. At the start of his period radicals sought to vilify and ridicule the new science of political economy. They thus followed the older radical tradition that saw "Old Corruption," taxation, and the national debt as the central cause of popular poverty. The core of his book is occupied by the proponents of a new analysis who sought not to vilify political economy but to use its own theoretical approaches to undermine its complacent and optimistic conclusions about the existing order of things. The key figures—Thomas Hodgskin, John Gray, William Thompson, and the rather later John Francis Bray—came to be known as the Ricardian socialists, and Thompson sees them as providing "the articulation and defense of working-class interests in the language of political economy" (p. 63). They rooted their analysis in a theory of value, without which a systematic critique of the new industrial capitalist economic system was impossible.

It seems that they have been falsely termed Ricardian socialists, however, for Thompson argues that their theories of labor exploitation were drawn

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from a Smithian rather than a Ricardian economic world. Whereas Ricardo saw no inconsistency between the value at which commodities were exchanged and their natural, labor values, Smith saw a deviation between the two introduced by exchange relations. In following Smith, the socialist political economists located exploitation at the point not of production but of exchange. Thompson draws two principal conclusions from this analysis. The first is that these Smithian socialists found exploitation within the economic system—a great advance on older radical analysis, which turned to an essentially noneconomic rhetoric of Old Corruption and natural rights. The second is that this advance was limited, because its analysis inevitably led to attempts to eliminate the distortions produced by the exchange process rather than to appropriate the means of production. The socialist economists thus concentrated on the reform of money, cooperation, equitable labor exchanges, and so on. This focus also led, according to Thompson, to the slide into reformist economic strategies and political goals in the decades that followed.

The book is an excellent piece of exegesis but in other ways is less satisfying. Thompson tends to assess the socialist political economists in terms of whether they were correct. The "weaknesses" he uncovers usually concern the direction of attention away from productive relations toward those of distribution and exchange. Yet he does not satisfactorily explore the implications of this tendency beyond stressing its erroneous character. He never considers whether this new economic analysis could be fitted into an older radical language concerned with middlemen, financiers, and the "unproductive," because the book is frustratingly introspective and, until the conclusion, fails to relate the ideas to the movements to which they were addressed. The repeated assertion that these were the ideas of the movement—"popularly accepted and popularly purveyed" (p. 222)—because he finds them in the newspapers is unconvincing. If they were so accepted, why did they so rapidly recede after 1834? Why the continuities with the Cobbett and Hunt radical tradition that are so evident during the 1830s and 1840s? The fascinating questions lead out of Thompson's careful study. What did popular radicals make of these ideas, how did they relate to older radical modes of thought, and why did they so concentrate on unequal exchange? The answer to this last question might be that this concentration made sense of an economic world that, for radical artisans, was being transformed through the capitalist reorganization of small-scale production by *merchant* capital. It is a shame that Thompson did not try to broaden his analysis to such larger questions, but his book remains important reading for students of early nineteenth-century radicalism.

GEOFFREY CROSSICK
University of Essex

GEORGE RUDÉ. *Criminal and Victim: Crime and Society in Early Nineteenth-Century England*. New York: Clarendon Press of Oxford University Press. 1985. Pp. 146. $42.95.

George Rudé is one of the outstanding pioneers of "history from below." Most notably, he charted the unknown territory of popular protest and ideology in eighteenth-century France and England: the Bastille crowd, the followers of John Wilkes, and the Gordon rioters. In recent years his interest in the history of the crowd has broadened into a desire to know more about ordinary crime and criminal justice. This shift of focus has done little to deflect his gaze from the human reckoning, as opposed to mere numbers. This book is a case in point: dutiful quantitative work is constantly enlivened by the details of individual cases. The result is a brief, expensive, yet readable assessment of crime and justice in a time of dramatic change. The book will have only limited appeal to historians of crime, who will be familiar with most of its conclusions.

Rudé tackles the central question of "who robbed whom?" by examining ten thousand criminal cases drawn from the original court records of three counties between 1800 and 1850. With little prior discussion of the evidential problems posed by the judicial statistics and by the inclusion of acquittals, he arrays the details of crime, criminals, and victims. In rural Sussex workmen in the building and service trades stole food and clothes from farmers and shopkeepers; in Gloucestershire industrial workers stole money, cloth, and animals from manufacturers and farmers. In Middlesex (essentially London) servants, shopworkers, pickpockets, and prostitutes stole money, valuables, and bedding from publicans and merchants. Significantly, the picture of London crime that Rudé sketches is at odds with that painted by Charles Dickens and Henry Mayhew. Rudé adduces little evidence of a criminal class inhabiting a criminal underworld. Judging from the number of prisoners pleading poverty as the cause of their crime, Rudé asserts that most crime, even in London, was "survival" crime.

The final two chapters on policing and punishment are less informative. They recount events and changes already well documented by historians. The role that victims played in the judicial process and the immense disparity in sentencing that fueled demands for legal reform are, however, striking. Few employers were eager to put a good, if light-fingered, workman behind bars, and so they refused

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to prosecute or provided a good character reference for the offender. As for disparity in sentencing, at the London assizes in 1820, the city recorder sentenced a fourteen-year-old lad to transportation for life for pickpocketing then gave a boy of the same age, guilty of stealing a five pound note from his master, a cloth dresser, a fine of one shilling.

Such details, drawn from the Old Bailey sessions papers (which provide richer pickings than most collections of English judicial records) make this book valuable. Rudé also provides a summary of the main findings in this relatively new field of social history, but the summary is based on a small sample of recent historical work. The book is well produced, but, in table 3.5 (p. 63), the figure of infanticide as a percentage of female crime at Gloucester assizes should be 8.4 percent, not 0.4 percent, and the Bow Street Horse Patrol is wrongly cited as House Patrol (p. 91).

VICTOR BAILEY
University of Hull

A. DWIGHT CULLER. *The Victorian Mirror of History*. New Haven: Yale University Press. 1985. Pp. ix, 320. $25.00.

ROSEMARY JANN. *The Art and Science of Victorian History*. Columbus: Ohio State Univerity Press. 1985. Pp. xxviii, 272. $27.00.

The point of departure of these books is the preeminence of history within Victorian high culture. Both A. Dwight Culler and Rosemary Jann emphasize that, although the didactic function of history was prized, the past was viewed not as a storehouse of exempla but rather in terms of the new historicism. Hence, by investigating the development of discrete cultures and societies, writers constructed usable pasts that enabled them to understand the present and cope with change within their own society. Jann confines herself to the work of historians, whereas Culler ranges more widely, including among his protagonists J. S. Mill, J. H. Newman, Matthew Arnold, John Ruskin, the Pre-Raphaelites, Robert Browning, and Walter Pater. Both authors discuss Thomas Arnold, T. B. Macaulay, and Thomas Carlyle.

Despite similarities, these two books complement rather than duplicate each other. Culler has written essentially an intellectual history of the Victorian period, since he asserts that varying interpretations of the past played a significant role in the Victorian dialogue on science, art, religion, and culture. After an initial chapter in which he describes how eighteenth-century England "looked into the mirror of history and declared itself an Augustan Age" (p. 10), Culler turns to Walter Scott and Macaulay. Culler concludes that the Tory novelist as well as the Whig historian subscribed to what Herbert Butterfield has defined as the Whig interpretation of history: both sought to trace movements in the British past that had shaped progress toward the present. In a chapter entitled "Mill, Carlyle, and the Spirit of the Age," Culler notes that these two very different thinkers shared the conviction that "the spirit of the age" resulted from an inexorable historical process and that solutions to current political and social problems depended on a proper reading of the "spirit" of their own time. Thomas Arnold also turned to history to gain perspective on the crises of early nineteenth-century England. He argued that modernity was not a function of chronology: the age of Thucydides and contemporary England were equally modern and therefore beset with similar problems. Perhaps the most interesting aspect of the chapter on Newman and the Tractarian movement is Culler's clear exposition of Newman's *An Essay on the Development of Christian Doctrine* (1845), which "introduced change and history into an unchanging science" (p. 106). After discussion of Matthew Arnold's three-stage philosophy of history and its connection with his views on politics, religion, and culture, a series of chapters focus on Victorian medievalism and the Victorian Renaissance. Discontent with the present was the common denominator of Victorian medievalism, which could take conservative, Whig, or socialist forms. Ruskin and the Pre-Raphaelites were the heirs of this complex and frequently contradictory tradition, rejected by Browning and Pater in the name of values they associated with the Renaissance. Bald summary does not do justice to the extraordinary density of this book, achieved through deft comparison as well as seemingly effortless erudition.

Jann identifies a distinct genre of Victorian historical writing, one that combined the appeal and authority of both "art" and "science." She examines the work of six representatives of this genre: Thomas Arnold, Carlyle, Macaulay, J. A. Froude, J. R. Green, and E. A. Freeman. These historians thought of themselves as scientific: inductive in method and capable of establishing laws that would use the past to interpret the present and future. Yet they were literary in their insistence on the role of imagination in the creation of historical narrative, concern for stylistic felicity, and desire to reach a wide audience. Scientific and literary components were linked by didactic intent. Jann's careful analysis of historiographic texts demonstrates that particular stylistic strategies enabled each historian to communicate his message to a prospective audience. The chapters on Froude, Green, and Freeman develop one of the major themes of the book: the impact of the new professional history on the older model of Victorian historiography. In his notorious polemic against Froude's work, Freeman spoke as the cham-

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pion of professionalism; nevertheless, Freeman's own historical writing was deeply imbued with traditional values. By examining Green's career in terms of "his efforts to accommodate the popularizing impulse of Victorian history to the increasingly incompatible demands of the new scholarship" (p. 142), Jann has added an important dimension to the familiar figure of the Whiggish social historian.

In an epilogue Jann sets herself the task of appraising the late nineteenth- and early twentieth-century quarrel between the adherents of "the amateur and professional traditions" (p. 216). Real issues of status and authority lay beneath the surface of exaggerated charges and countercharges. Should the public accept the moral and aesthetic criteria of literary history or the research-oriented standards of the new academically based professional historians? She concludes that, despite the inroads of professionals, English historiography has to a large extent preserved the values of the Victorian historians who considered themselves "both sage and scientist" (p. 214). This is surely unexceptionable. The epilogue is, however, open to criticism. It is unduly schematic: Jann assumes that historians held clearly defined positions in a debate that continued from the 1870s well into the twentieth century. In concluding that in England many of the virtues of the amateur tradition have been retained, Jann makes much of the acceptance of the value of history as a humanistic study by professional historians from William Stubbs to R. W. Southern. But the significance of this point is obscure, since professional historians elsewhere have not found themselves in disagreement with Stubbs and Southern. A rather startling misconception of the professional ethos would appear to be at work here.

Taken together, these two books present the reader with a definitive account of history among the Victorians: of its practitioners, its audience, and of the ends it was meant to serve.

DORIS S. GOLDSTEIN
Yeshiva University

LUCY BROWN. *Victorian News and Newspapers*. New York: Clarendon Press of Oxford University Press. 1985. Pp. 303. $55.00.

Newspapers are an important subject in nineteenth-century British history. As one of the chief means of public communication, the press both reflected and shaped opinion. Lucy Brown's book is a well-researched study of the process of providing the news.

The title is, however, somewhat misleading because Brown is concerned almost exclusively with the latter part of the century. Most of her evidence is from the era after 1870, when the newspaper was already an established institution. Unfortunately, this perspective diminishes the significance of such critical matters of earlier decades as the effects of the stamp and other duties. Nonetheless, because of its time frame, the book is particularly good on the impact of technological changes, such as the introduction of the web rotary press, on production. Brown also traces, in considerable detail, patterns of circulation and distribution of London and provincial papers, patterns that were discernible by the 1880s. Her fitting treatment of late nineteenth-century journalism as a "specialized craft" (p. 80) leads naturally to a description of journalists, their backgrounds, techniques, networks, and sources. This information is a welcome addition to a field in which anonymity of authors has long been an impediment to investigation.

The work is less satisfactory in its chapters on the news. The definition of news, as "serious reports of public affairs, local and national" (p. 111), appears narrow, especially in light of the Victorian penchant for the sensational. Further, discussions of politics and foreign news, although richly documented, sometimes include unoriginal generalizations. For instance, it is hardly surprising that, once the Reform Act of 1867 had passed, the newspaper could be "a magnet for party loyalty" (p. 68); or that, in the world of Bismarck, "the coverage of foreign events reflected the diplomatic map of Europe" (p. 242). Moreover, the examples cited to explain a category of news occasionally reveal a curious emphasis. As an illustration, in the chapter "Political News," eighteen of thirty-nine pages deal with Gladstone whereas Disraeli is mentioned only twice.

Minor problems of style include the tendency to write in the first person plural and textual references to later chapters without corresponding footnotes of direction. But these flaws are more than offset by first-rate indexes, one devoted to periodicals, and a thorough bibliography comprised of manuscripts, documents, contemporary materials, and an extensive list of metropolitan and provincial gazettes.

In summary, this book makes a valuable contribution to the record of the Victorian press. Brown complements the work of Alan J. Lee and the late Stephen Koss by focusing on a time when many papers were trying to be news intensive, an interesting, albeit infrequent, priority of the medium for much of the nineteenth century.

E. M. PALMEGIANO
Saint Peter's College

ELAINE SHOWALTER. *The Female Malady: Women, Madness, and English Culture, 1830–1980*. New York: Pantheon. 1985. Pp. xii, 312. $19.95.

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Elaine Showalter, a feminist literary critic, has set out to write a "feminist history of psychiatry and a cultural history of madness as a female malady" (p. 5). Analyzing medical and literary texts as well as photographs and paintings, she traces the conception and treatment of women's insanity through three phases of English psychiatry: psychiatric Victorianism (1830–70), psychiatric Darwinism (1870–1920), and psychiatric modernism (1920–80). Showalter's central premise is that a "feminization" of madness took place in the nineteenth century; women not only became the primary recipients of psychiatric treatment but also served as the cultural exemplars of madness. She also argues that, until very recently, psychiatrists treated female patients within the context of a restrictive femininity that was itself the origin of their psychological demoralization. Showalter concludes that women have begun to break "the chains that make madness a female malady" only in the past few decades, with the rise of a feminist therapy movement (p. 350).

The book makes a powerful case for the influence of gender on psychiatric conceptions and treatment of female patients. But Showalter pushes her argument too far in according the madwoman such an exclusive hold on the psychiatric imagination. Her perspective greatly exaggerates the extent to which psychiatrists have viewed insanity as a peculiarly female complaint or regarded women as their primary clientele. (One suspects she is projecting backward the relatively recent tendency for women to seek psychotherapy in far greater numbers than men.) Given the numerical predominance of women over men in the general population and the tendency of women to live longer, the slight majority of women in nineteenth-century English asylums—54 percent in 1872, according to her own data—hardly seems to constitute a "femininization" of the asylum. Further, nineteenth-century psychiatrists differed considerably more over the relative liability of the sexes to insanity than Showalter's analysis implies.

Ironically, in a book about women and madness, the best chapter focuses on male "shell-shock" victims in World War I and the female novelists who appropriated the soldiers' experience as a symbol of their own psychic helplessness and repression. But Showalter's assertion that "the Great War was the first, and, so far, the last time in the twentieth century that men and the wrongs of men occupied a central position in the history of madness" suggests a highly selective and ultimately inaccurate reading of the sources (p. 194).

Showalter's considerable skills as a literary critic are simply unequal to the complexity of her topic. She is at her best when treating the cultural representations of female madness, such as the Ophelia convention in psychiatric literature, Hugh Diamond's highly stylized photographs of female asylum inmates, and women's fictional accounts of madness. When she moves from cultural representations to the reality of psychiatric institutions, her analysis falters. Still, Showalter's work raises valuable questions about the power of gender as a determinant of psychiatric treatment. However much one may disagree with some of her conclusions, Showalter's book should provoke a fresh examination of this significant but neglected topic.

NANCY TOMES
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SAMUEL COHN. *The Process of Occupational Sex-Typing: The Feminization of Clerical Labor in Great Britain.* (Women in the Political Economy.) Philadelphia: Temple University Press. 1985. Pp. viii, 279. $34.95.

Perhaps you remember comedian Sam Levinson's best story: unexpected company for dinner had his mother herding her large family into the kitchen. "When you're asked if you want chicken" she said, "just say, no thank you, that you don't like chicken." The children behaved dutifully. Later his mother brought in the pie, and declared triumphantly, "all those who didn't have chicken, can't have dessert."

For decades the General Post Office (GPO) in Great Britain voluntarily extended state protectionist legislation intended for factory workers to its clerical staff and refused to employ women on the night shift. But men—understandably—did not appreciate the graveyard shift either. So the reward for night work was transfer to special day positions—and here is the Levinson phenomenon in action—only those who had worked nights were eligible for promotion. This strategy played only a bit part in a much larger process that feminized clerical work, kept wages down, and systematically deprived women of promotion. Samuel Cohn has researched this process meticulously for two British corporations, the private Great Western Railway and the GPO.

This study is not for the statistically faint-hearted nor for those wanting easy answers to questions about gender job ghettoes, for caveats abound in this careful study. Nor is it for those seeking confirmation of human capital theory or labor process theory: both are put to empirical test and rejected. Also denied is the role of male workers in blocking women's entry and promotion. Not workers but managers have this power, which they exercise especially to control entry to their own ranks.

Cohn instead elaborates the "synthetic turnover thesis." The expanding need for clerical workers experienced in the GPO as early as the 1870s brought new problems of management. Acquisition

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of skills reached a plateau in clerical work after a few years and made increases in salaries unprofitable. Yet long-term workers expected periodic increments—as well as promotions, which were also at a premium. A reasonably rapid turnover of well-educated workers would have solved the problem as resignations made possible the hiring of cheaper, entry-level employees. The solution was to hire women who would leave their jobs to marry. But time showed the flaw in this reasoning, for the women married—and stayed. Management responded with bars on marriage and dowries. This solution was more successful, although some women delayed, or even avoided, marriage in response.

Cohn demonstrates that through bars on marriage the profit motives of capitalists and the patriarchal attitudes of managers together created segregated and unequally rewarded work forces. When profit and sexist motives clashed, desires for profit won out. But most of the time the marriage seemed made in heaven. Cohn's study then is also a valuable contribution to the research on the complex relations between class exploitation and gender oppression.

ROBERTA HAMILTON
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L. MARGARET BARNETT. *British Food Policy during the First World War*. Boston: Allen and Unwin. 1985. Pp. xix, 241. $35.00.

Bread, sugar, meat, and other food products were important items on the shopping list of British officials from 1914 to 1918. Officials pondered the policy questions of how the items could be acquired at reasonable prices and distributed equitably among the civilian populace. Efforts to devise satisfactory answers to the issues of food resources were made by many agencies, especially the Ministry of Food.

L. Margaret Barnett charts in great detail the progression of food control from prewar planning to wartime application. She thus offers some new interpretations and substantially fleshes out a recent article on the subject (see José Harris, "Bureaucrats and Businessmen in British Food Control, 1916–1919," in *War and the State*, edited by Kathleen Burk [1982]).

Barnett argues convincingly that change in the official policy of laissez faire occurred earlier in the war than has generally been assumed. By 1915 the Liberal government had become at least partially responsible for the acquisition of wheat, meat, and sugar and had taken tentative action on prices and shipping. These actions and subsequent ones later in 1915 provided strong foundations for policies devised after 1916 by the government of David Lloyd George. Barnett also suggests that Lord Shelborne at the Board of Agriculture, Walter Runciman at the Board of Trade, and Lord Devonport at the Ministry of Food have been depicted incorrectly by historians as unalterably opposed to governmental intervention in food supply. The issue for them, Barnett demonstrates, was over the methods for intervention rather than over intervention as such.

Besides showing the origins of the food control policies, Barnett discusses two other determinants of food and policy, especially after 1916: the influence of Hoover and the United States Food Administration and the pressure from the Labour party and consumer advocates. One of Hoover's prime aims in his push for inter-Allied trading was to force Britain to change its policy of stockpiling reserves, a change averted only by the timely end to the war. But, if Hoover's pressure for changes in British food policies were kept at bay, this was not the case with Labour's demands.

In January 1918 the Consumer's Council was created in order to contain growing Labour unrest about low food supplies and high prices by bringing Labour representatives into government advisory councils. For Labour this entrée provided an opportunity to criticize thoroughly the possible effects of the proposed food policies. Barnett argues that the result of this type of participation and Labour's concern for consumer rights greatly helped make socialism palatable to many. This may be true, but the argument needs more substantiation.

Another flaw is Barnett's lack of reference to the impact of the imperialists' ideas about food policy. Although she discusses the problems with shipping food from parts of the empire, the author does not analyze the ideas of those who tried to use the emergency of the war to push for control of food supplies as a means of creating a self-sufficient empire.

These reservations notwithstanding, the study is a very solid contribution to the literature on British policies during World War I.

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ROBERT SKIDELSKY. *John Maynard Keynes*. Volume 1, *Hopes Betrayed, 1883–1920*. (Elisabeth Sifton Books.) New York: Viking. 1983. Pp. xxviii, 447. $24.95.

The task confronting a biographer of John Maynard Keynes is formidable. Keynes's life was not only important—no British thinker in our century has had as much influence on the course of history—but complex and manifold. Keynes was an

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immensely active public man who nonetheless lived an intense and unconventional private life. Moreover, he moved at the center of a multiplicity of worlds—Cambridge, Whitehall, the City, and Bloomsbury. His biographer must be not only equally at home exploring public and private life but also conversant with the workings of these different social worlds. Robert Skidelsky, author of *Politicians and the Slump* (1967) and *Oswald Mosley* (1975), passes these tests with exceedingly high marks.

Hitherto, we have been given only the public Keynes who reconstructed economics and advised governments. Skidelsky shows how much Keynes's first biographer, Roy Harrod, left out, in part from lack of interest but chiefly from a zeal for respectability. Suppressed was anything that might tarnish or trivialize Keynes's sober public image and thus be useful to the opponents of Keynesian policies—most notably his promiscuous homosexuality but also his application for conscientious objector status in World War I, his early financial speculations, and, more generally, his continuing "Bloomsbury" irreverence toward accepted moral standards. One of the most interesting aspects of this book for the professional historian is its exposure of Harrod's suppressions and distortions—to some degree understandable given the moral and political climate in which Harrod was writing but nonetheless a caution against uncritical reliance on biographers with their own agenda.

Skidelsky does more, however, than sensitively restore Keynes's private world (which has already been emerging in the outpouring of writing on Bloomsbury set off by Michael Holroyd's *Lytton Strachey* [1967]). He uses knowledge of Keynes's private life to enlarge our understanding of the public Keynes. Skidelsky's vivid account of Keynes's life among the turn-of-the-century Cambridge "Apostles," with their intellectual elitism and their apotheosis of personal relations as the arena for working out the "good life," throws a fresh light on his postwar technocratic-cum-libertarian reconstruction of liberalism (as analyzed recently in Michael Freeden's *Liberalism Divided* (1986). And, in recounting, without prurience, the homosexual love life that was more important to Keynes than his public activities, at least until the war, Skidelsky makes more comprehensible his subject's unusual fusion of "establishment" and "outsider" stances.

Skidelsky is not only equally assured in delineating Keynes's private and public involvements but also at home in all of Keynes's many worlds. He analyzes "Cambridge civilization" and, later, the Bloomsbury outlook on life with the same lucidity and insight he brings to a discussion of the quantity theory of money, the workings of Whitehall, or the issue of war reparations. The reader is helped by this not only to see Keynes in the round but to reflect on possible connections between the collapse of "Victorian values" and the emergence of new approaches to public policy.

Beyond stressing the centrality of personal life, Skidelsky reshapes our picture of Keynes in several ways. He stresses Keynes's lack of interest before World War I in social reform, and even in politics, and his slowness to commit himself to economics as his life's work; Keynes regarded economics as "an inferior branch of knowledge—inferior to philosophy in which he still hoped to make an original contribution to thought" (p. 229). This changed with the war, which Skidelsky shows to be even more of a watershed in Keynes's life that we had supposed. Called to the Treasury, he rapidly rose to a position of major influence, from which he derived enormous satisfaction, while, however, he watched with increasing horror as millions of lives and the national economies of Europe were consumed. The war opened up a vast field for the exercise of his gifts and simultaneously rendered untenable his sanguine world view of a steadily unfolding process of "civilization," the last legacy early Bloomsbury had inherited from Victorianism. This prospect of "civilization" had rested on a base of economic stablility and progress that was taken for granted. For Keynes, the war had exposed the fragility of this base and shown the urgent need to reconstruct it. Henceforth, "prosperity would have to be strenuously won in the teeth of the adverse circumstances which the war had created" (p. 400). Ironically, without this "betrayal" of his early hopes, the Keynes who has so affected our age might never have emerged out of the privately radical but publicly conventional Edwardian so ably portrayed here. I hope we will soon be observing this emergence in the second, concluding volume of this distinguished biography.

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CHRISTOPHER ANDREW. *Her Majesty's Secret Service: The Making of the British Intelligence Community.* (Elisabeth Sifton Books.) New York: Viking. 1985. Pp. xviii, 619. $25.00.

Intelligence studies are enjoying a renaissance. Their popularity reflects both a reaction against the criticism of intelligence and the Central Intelligence Agency of the 1970s and a powerful belief that better intelligence is essential to preserve Western interests against mad Mullahs, crazed Libyans, and that conveniently amorphous entity, "state-sponsored terrorism." The barely concealed political assumptions underlying this revival account for the shallowness and poverty of much of its product, which too often breaks the fundamental principle of intelligence by reaching conclusions before having

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examined evidence. The attitude is also often dangerously unhistorical. But Christopher Andrew's book reminds us that good work is also done in this field. Moreover, history can help us learn from experience and save us from extravagant expectations.

Andrew's book, in the making for over a decade, shows that a careful scholar, skillfully searching through private and public documentary collections (and with the help of good connections), can write credible intelligence history. As David Dilks has argued elsewhere, intelligence is "the missing dimension of most diplomatic history." Add to that military and strategic history, and Andrew's book becomes essential reading for all who would better understand British foreign and defense policy since the heyday of Victorian hegemony. In particular, it illuminates some of the darker corners of British foreign policy in the years before World War II. But Andrews concentrates on the development of the British intelligence community, and the real fascination and value of the book lies in its portrait not only of that community's growth but also of its strengths, weaknesses, and pathologies.

Before World War I spy mania, fostered by fears of the German naval challenge to British diplomacy, led to the creation of what we now know as M.I.5 and M.I.6 and along the way created vastly inflated estimates of the German espionage menace. After the Bolshevik revolution the face of fear changed, and equally extravagant claims about Soviet subversion preoccupied the spycatchers. Too frequently, they projected their class fears and prejudices onto obvious targets and missed the real problems and villains. But, when presented with wars that cauterized fantasy and improved coordination, British intelligence performed well. During World War II, with triumphs such as "Ultra" and the "Double Cross system" to their credit and under the inspirational leadership of Churchill, the fragmented British intelligence services finally coordinated their efforts and became a mature and professional community. Oddly enough, Andrew's book says little about the wartime period and ends instead with a familiar plea for the breaking of secrecy taboos that have surrounded British intelligence activity since the beginning. By revealing how much he has discovered already, Andrew's book is a weapon in that cause. It should also provide a historical caution to those who worship at the shrine of intelligence. Religion fills emotional needs. It is rarely good for the formulation of public policy.

DAVID STAFFORD
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D. J. MORGAN. *The Official History of Colonial Development.* Volume 1, *The Origins of British Aid Policy, 1924–1945*; volume 2, *Developing British Colonial Resources, 1945–1951*; volume 3, *A Reassessment of British Aid Policy, 1951–1965*; volume 4, *Changes in British Aid Policy, 1951–1970*; volume 5, *Guidance towards Self-Government in British Colonies, 1941–1971.* Atlantic Highlands, N.J.: Humanities Press. 1980. Pp. xxx, 253; xiii, 398; xvii, 334; xv, 275; xvii, 382.

The term "official history" is inevitably chilling for too often it implies an apologia dully conceived by a less than competent historian provided with privileged access to records denied other scholars. Happily, the five-volume history of British colonial development in the years 1924–70 is, in large measure, an exception. Written by D. J. Morgan, an academic economist with broad international and colonial experience, it is indeed exhaustive but also analytical.

Volume 1 is devoted to the origins of British aid policy between 1924 and 1945. The prologue discusses the growth of the concept of British funding for colonial welfare and development. Most significant are the discussions of the conditions leading to the passage of the Colonial Development and Welfare Acts of 1940 and 1945 and the effects of these ground-breaking pieces of legislation.

Volume 2 deals with the development of colonial resources in the years 1945–51. Particular attention is directed to the workings of the Colonial Development and Welfare Act of 1945, the allocation of grants in aid and loans, and the inception and operation of the Colonial Economic Development Council. Special emphasis is accorded the West Indies and the notorious East African groundnuts scheme. A final section discusses the creation of the Colonial Development Corporation and some of the plans formulated under its sponsorship, such as the Gambia poultry scheme.

The third volume of this extensive history concentrates on the reassessment of British colonial-assistance policy in the years 1951–65. Although the British commitment of funds to colonial development cannot be judged as overly generous, it was not niggardly if financial conditions in Britain itself are taken into account. Between 1948 and 1959 the Colonial Development Corporation spent 64 million pounds, and governmental developmental and welfare expenditures from 1946 to 1958 came to 155 million more. Loans from the London market for the period 1946 through 1957 amounted to 187 million pounds. But it must be noted that local government resources provided 600 million pounds for development and welfare over the same number of years.

The emancipation of the colonies transformed colonial into overseas development, which came to be governed by the Commonwealth Development

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Act of 1963 and the Overseas Development and Service Act, passed two years later. The Commonwealth Development Act made available only two-and-a-half million pounds of new money.

Volume 4 emphasizes changes in British overseas aid policy from 1951 to 1970. Decolonization forced further modifications in policy, both semantic and real. Thus, the Department of Technical Cooperation turned into the Ministry of Overseas Development, which was forced to establish new methods of assisting former colonies after independence. In this context special attention is paid to the West Indies as an early model for regional economic planning. Not surprisingly, the Colonial Development Corporation became the Commonwealth Development Corporation in 1963, and the changing pattern of its activities is the focus of the last part of this volume.

The final volume of the work is probably the most useful. In it the author attempts to synthesize the course of British assistance to the colonies and former colonies in the period from 1941 through 1971. Although the treatment remains obdurately chronological, the discussions of such topics as decolonization and constitutional change, the evolution of the Commonwealth, and the fate of the smaller colonies are perceptive and deeply informed. Although this work essentially is of value to the specialist, the final volume will be useful to the more general historian.

ROBERT A. HUTTENBACK
University of California, Santa Barbara

BRUCE R. GALLOWAY and BRIAN P. LEVACK, editors. *The Jacobean Union: Six Tracts of 1604*. (Scottish History Society, fourth series, number 21.) Edinburgh: Clark Constable, for the Scottish History Society, Edinburgh. 1985. Pp. lxxx, 268.

In 1603 Englishmen enthusiastically welcomed the accession of a king from Scotland. But they most emphatically did not welcome his countrymen or any close relation with their realm. Scotophobia ran remarkably deep, and James's projected union of the two kingdoms caused far more tension between him and his first Parliament than did any other issue. Many Scotsmen also greeted the prospect of union with considerable reluctance, but they did not reciprocate their neighbors' visceral contempt, and this circumstance often enabled them to respond more creatively and imaginatively.

Not surprisingly, James's aspirations and their subsequent frustration stimulated much comment and more than two dozen formal tracts on the subject. For too long only the most significant of them—those by Francis Bacon and Thomas Craig—were available in modern editions. Now Bruce R. Galloway and Brian P. Levack have substantially enhanced our access to this important confrontation between the two English-speaking cultures by publishing six additional tracts, five of which have never previously appeared in print.

Three of the authors are Scots, ranging from Robert Pont, one of the fathers of the Scottish Reformation, to John Russell, probably an Edinburgh lawyer, to an altogether anonymous writer. The eminent scholars Henry Savile, Henry Spelman, and John Doddridge represent the English side. The Scottish tracts are consistently more religious—Russell ascribes profound apocalyptic significance to the union—whereas the southerners display an overwhelming legal-antiquarian interest. If the Scots feared their country might become a mere "pendicle" of England—and if they insisted that in any crisis James's first commitment lay with his native land, the English enterprise being merely "accessory"—they visibly had an enthusiasm for union unshared by their counterparts.

These writings consequently provide many of the more salient dimensions of the debate about union. In addition, the editors' comments on the legal implications of union and on opposition to it within the House of Commons are especially illuminating. And yet the Scottish voice, lacking the rich archival materials of the south, is far more elusive, and that the Scottish History Society chose not to devote more space to its recreation is surprising. It is regrettable, for example, that the editors excluded David Hume of Godscroft's two-part *De Unione*. Hume, regarded by his contemporaries as the intellectual successor to George Buchanan, wrote the only piece to be stopped in the press. He believed that Scottish liberty could only be assured by exporting Presbyterianism to England—in several ways anticipating Scottish opinion in 1643. Similarly, the editors unfortunately chose to print a contemporaneous English translation in "the spirit" of Pont's *De unione Britanniae dialogus* rather than the text with a modern rendering. In 1909 C. S. Terry surely set the standards when he produced Craig's *De Unione* with translation. We must all regret that—presumably for fiscal reasons—the Scottish History Society can no longer maintain these standards.

None of this denies the editors' enormously useful service. James would certainly have approved the "balanced" selection, if not the tracts themselves. To read them together, however—especially under the auspices of the Scottish History Society—only further illustrates that, then as now, "Britain" is a curiously Scottish phenomenon.

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SAMUEL CLARK and JAMES S. DONNELLY, JR., editors. *Irish Peasants: Violence and Political Unrest, 1780–1914*. Madison: University of Wisconsin Press. 1983. Pp. xiii, 454. $35.00.

This collection of original essays provides an impressive display of the "new" Irish history that has made this field increasingly important. The ten essays are arranged in three sections: "The Tradition of Violence," "Land and Religion in Ulster," and "Changing Lines of Cleavage and Cohesion." Samuel Clark and James S. Donnelly, Jr. also provide a penetrating analytical introduction that places the articles in a wider European context.

Three of the strongest essays deal with prefamine agrarian violence. In a careful argument David Miller illustrates the value of reexamining familiar historical events through the prism of new theoretical constructs. By testing breakdown and solidarity theories against his empirical evidence, he offers new insight into the sectarian violence of late eighteenth-century Armagh and illustrates the complexity of apparently "simple" rural events. Paul Roberts, in an impressive empirical examination of factional fighting, demonstrates that this violence, traditionally considered "recreational" or "primitive," was actually an effort by the poorest members of society to impose an alternative economic system on an increasingly capitalistic rural world. Donnelly persuasively argues that millenarianism played an important role in overcoming class differences within the Catholic peasantry and gave them a sense of hope, which Daniel O'Connell used effectively in his movement for emancipation.

Two articles deal with the critical period in the formation of modern Ulster politics. In examining the dynamics of elections in Ulster from 1868 to 1886, Brian Walker provides much useful information. Unfortunately, his article demonstrates one of the dangers inherent in a "revisionist school." His determination to deny the traditional characterization of the political relationship between landlords and peasants as coercive is unconvincing. His only "evidence" is the decision of one court case in a landlord's favor. Oddly, the only testimony quoted indicates coercion was indeed a factor in Ulster elections. A more analytical examination of the same subject by Paul Bew and Frank Wright provides a coherent explanation of the restructuring of Ulster politics by analyzing the confusing crosscurrents within the Protestant community created by conflict between landlords and tenants and the pan-Protestantism facilitated by the reassertion of dominance over a mobilizing Catholic majority.

David Jones, in the most intriguing essay in the collection, explores the widening gap between wealthy graziers and the remainder of the rural community. By combining an economic analysis of the shifting market influences on the production of agricultural commodities with an examination of the social and cultural milieu of the rural community, Jones raises interesting questions about the relationship between class and identity in rural Ireland. Other articles by David Dickson, William Fiengold, John Boyle, and Líam Kennedy cover, respectively, taxation and discontent, local elections and democratization, the marginal nature of the agricultural laborer, and the complex political relationship between farmers and the townspeople who serviced them. The conclusion of the volume offers a thoughtful agenda for future work.

As a whole, the volume demonstrates the wealth, complexity, and sophistication of Irish rural studies. The book is essential reading for anyone involved in modern Irish history. It will also serve as an excellent introduction to this rich field for scholars of other peasant communities and all interested in problems of economic and political development.

KEVIN O'NEILL
Boston College

LÍAM KENNEDY and PHILIP OLLERENSHAW, editors. *An Economic History of Ulster, 1820–1940*. Dover, N.H.: Manchester University Press. 1985. Pp. 248. $26.00.

Líam Kennedy and Philip Ollerenshaw have sought to produce more than a book of readings. This volume is intended to stand as an integrated "introductory text." In seven "chapters" written by specialists, it covers subjects such as the rural economy, industry, transport, population change, urbanization, and the labor force. I most liked chapter 1, Kennedy's examination of the rural economy from 1820 to 1914, in which he provides an excellent synthesis of the literature and fine discussion of proto-industry. But all the chapters are good. They are written by people who know their subject well, and the editors have little reason to complain about their contributors.

Yet the question can still be asked: how well do seven authors succeed in producing an integrated text? As criteria of an integrated text I suggest the following. All important subjects should be covered; each chapter should build on those preceding it; repetition and contradiction should be kept to a minimum; authors should indicate an awareness of the contents of other chapters and should make appropriate cross references; and the book should have a unifying theme.

This book is reasonably comprehensive but suffers from several serious imbalances. Agriculture is neglected relative to industry, and the period 1820 to 1914 is covered more carefully than 1914 to 1939. Most chapters do not, I am afraid to say, build on those that came before. Although the authors have

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been able to agree on a division of labor, we encounter some repetition; for example, accounts of the growth of the textile industry appear in several chapters providing essentially the same information. And, except in Henry Patterson's chapter on the labor force and John Othick's concluding chapter, the contributors have not provided sufficient cross-references.

So far not so good. On the more positive side, however, the authors have avoided contradictions. More importantly, the book does have a theme, indeed several. First, throughout the book we find an emphasis on regional economic analysis; this emphasis is reflected not only in the provincial focus of the book but also in the sensitivity of the authors to regional variations within Ulster. Another theme is the importance of the relationship between the British economy and that of Ulster, a relationship that had both negative and positive consequences for Ulster; this subject is explored by several authors, though not discussed in enough detail. And, to mention one more, several chapters stress the "precariously narrow" industrial base of the Ulster economy and other weaknesses that have contributed to the economic decline of Ulster in the twentieth century. Othick's concluding chapter helps pull these themes together. Although he has a weakness for *non sequitur*, Othick makes a provocative and insightful contribution to the volume.

Altogether this is a fine book. Well-written, competent, and up to date, it will be useful to both scholars and students.

SAMUEL CLARK
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VOLKER ROESER. *Politik und religiöse Toleranz vor dem ersten Hugenottenkrieg in Frankreich*. (Basler Beiträge zur Geschichtswissenschaft, number 153.) Basel: Helbing und Lichtenhahn. 1985. Pp. 307. 75 FR.

Volker Roeser's study in essence illustrates a thesis drawn from a 1923 work by J. Kühn: an interrelationship exists between concepts such as religious tolerance or intolerance and sociopolitical needs in actual historical circumstances, and notably in the sixteenth century the Christian humanist version of tolerance had much to offer secular authority, at least potentially. Having explained these antecedents, Roeser applies the thesis to the reign of Francis II, because this monarch's government—and particularly its leading spirit, the Guise cardinal of Lorraine—found it politically useful to substitute a policy of toleration for one of persecution in dealing with religious dissent. This toleration was conditional, based on Christian humanist emphasis on practical piety and church reform. Christian humanism did not accept permanent confessional differences or liberal individualism but rather, in the tradition of Erasmus and Castellio, denied the usefulness of force in persuading consciences and called for tolerance of diverse opinions pending the resolution of disputes about doctrine and discipline by a church council. The goal therefore was eventual unity, not pluralism.

With a careful reading of sources, some in manuscript but most printed (memoirs, correspondence, diplomatic reports, statutes, and so on), Roeser traces Lorraine's policy of Christian humanist toleration in France and in Scotland, which the Guises regarded as a French sphere of influence under the regency of their sister Marie. In both Scotland and France Calvinism proved impossible to root out, and the type of toleration advanced by Christian humanists seemed to offer the French government a means of containing factions. The policy did not get much of a trial in Scotland, where English support stiffened the resistance of Protestant nobles. And in France itself, with the death of Francis II and the consequent shift in political constellations, the provisional toleration proclaimed by the edicts of Amboise and Romorantin in 1560 came to nothing, and hopes of religious unity faded with the failure of the Colloquy of Poissy and the refusal of the French Catholic clergy to compromise on the doctrine of salvation or to act as a national reform council. Christian humanism as an instrument of politics therefore did not last long, although Roeser contends that it was doomed by circumstances rather than by any inherent flaws. Beyond elaborating on Kühn, the point this proves is difficult to see, since Roeser does not place Christian humanism or humanist toleration in a larger historical perspective. His work is thus an academic exercise that may be useful to other historians mainly as an invitation to reexamine the political and religious role of the Guise family.

The book, which is well written, has a bibliography, an index, and extensive notes.

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ROBERT MUCHEMBLED. *Popular Culture and Elite Culture in France, 1400–1750*. Translated by LYDIA COCHRANE. Baton Rouge: Louisiana State University Press. 1985. Pp. 326. $30.00.

The original French edition of Robert Muchembled's sociocultural history of early modern France, published in 1978, has been translated by Lydia Cochrane into an excellent English rendition. Muchembled holds that popular culture in France was systematically repressed during the seventeenth and early eighteenth centuries by the combined forces of

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state and church. He argues that from 1400 to 1600 a cohesive system of popular culture, which was built on local custom, collective life (family, clan, village), and magical animist visions of the world, flourished in a decentralized political system. When subjected to repeated and brutal attacks by the new centralized state, however, popular culture withered and declined between 1600 and 1730. Following the dictates of the absolutist state, which developed a fearsome "political technology of the body," and the Counter Reformation church, which imposed a guilt-ridden religious sensibility, human bodies and souls were physically and spiritually constrained in the name of social conformity. Along the way the marginal persons who remained tied to the popular vision and might have revolted during crises were brought to heel by the examples afforded in witch trials. Since witches (who were mostly women) perfectly fit the norms of popular culture, their avid persecution between 1580 and 1680 bore witness to the mission instigated by the elites—to acculturate and Christianize the world of popular culture.

This cultural conquest was completed when the dominant elites (the court, the lettered, the nobility, and the urban bourgeoisie) successfully disseminated their own ideology, which fostered conformity at the expense of diversity and obtained a kind of fictitious consent from the masses. Many adherents of popular culture, pauperized and subjected by the absolutist state at the end of the seventeenth century, imbibed the new "mass" culture that was made to measure for them and disseminated through royal and religious imagery (the monarch, the crucifix) and by peddlers' literature (the *bibliothèque bleue*). Far from serving as a countercultural phenomenon, this new mass culture popularized the ideological agenda of the dominant elites and dealt a fatal blow to the popular vision. At this juncture elite culture and popular culture, now mediated by mass culture, ceased to have direct contact. Muchembled finds that the underlying causes of this sociocultural transformation were rooted in the changing economic infrastructure (the movement from feudalism to capitalism), the political structure (the organization of the absolutist monarchy), and the mental superstructure (the spread of militant Christianity). On these grounds three sociocultural strata formed (the first two linked antagonistic partners in the macroculture, the third remained isolated in a microculture). The first, a minority high culture of elites, consisted of the men of letters, scholars, and philosophers from the upper bourgeoisie and liberal aristocracy, who were educated in the *collèges* and extolled reason and progress while blocking social mobility. The second, a majority mass culture of aspiring elites, consisted of the middle bourgeoisie, minor officials, and local notables, who adopted the dominant ideology of the elites and separated themselves from the popular world below. And the third was a marginal popular culture that comprised persons of the lower classes and rural peasantry, who remained separated from the rest of society. Until the prerevolutionary period this veiled transmission of absolutist ideology and militant Christian doctrine to the majority supported the status quo in France and disallowed a true class struggle.

Muchembled's interesting and lively presentation paints a rich picture of the multiform and magical world of popular culture in the fifteenth and sixteenth centuries. For many historians of early modern Europe, however, the dialectical process posed here, juxtaposing this beleaguered pristine popular world in the seventeenth century with its nemesis, the dominant authoritarian elite culture, is far too rigid and much too dependent on the conceptual frameworks of Michel Foucault and Jean Delumeau. Others will object to the notion that elites perpetrated a mass culture, or elite ideology, meant to tranquilize the lower classes and will maintain the opposing interpretation (associated with Robert Mandrou and Roger Chartier) that the new culture served countercultural purposes. Still others will ponder the positive, rather than negative, effects of constraining bodies in a system in which willful rape and violence against women were features of everyday life. Finally, historians will continue to assess with proper weight the crafty and intelligent strategies of the rural peasantry and the urban poor, who resisted such pressures then and now.

SARAH HANLEY
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CHARLES TILLY. *The Contentious French*. Cambridge: Belknap Press of Harvard University Press. 1986. Pp. xiv, 456. $25.00.

Historians and sociologists ought to have much to say to each other, yet in practice they rarely look across the barrier that separates them, and, when they do, they often tell themselves that the alien landscape seems rather barren. In our time the sociologist Charles Tilly has been the most notable leveler of barriers, for in his work he fuses the purposes of the sociologist with the methods of the historian. His new book is vintage Tilly. The wealth of sources should impress any historian: the book reflects years of digging through departmental and national archives, together with selective soundings in periodicals and manuscript collections. Pleasing likewise to historians is the writing style, which is cogent and lucid, quite free of jargon. The goal, however, is to generalize about social behavior and social change, and not to describe or narrate.

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The book's title, like many book titles these days, is a bit misleading. One might expect *The Contentious French* to be a study of French national character, but the subtitle quickly puts us straight: the book is concerned with the record of French popular protest over the last four hundred years. More specifically, Tilly advances the hypothesis that the growth of capitalism and the process of statemaking fed the discontents that produced group protests, and he sets out to show how those processes affected the content and forms of collective action.

Not surprisingly, both the issues and the forms changed over time. In the seventeenth century, tax rebellions were most common; in the eighteenth, grain seizures (which remained dominant until about 1850); and, after this latter date, the focus became conflicts between labor and capital as well as collective efforts to control the state and its resources. Before the mid-nineteenth century protest remained largely parochial; thereafter, it became more often national. Meanwhile the forms and the social character of protest were also evolving. At the outset it crystallized out of traditional local institutions and rituals (deliberative assemblies, serenades, charivaris), and it often enjoyed the patronage of the traditional social elites. By the end of the seventeenth century protest had begun to involve direct popular action by plebeian crowds and had lost aristocratic support. From the mid-nineteenth century onward most protest was organized by parties, unions, and professional associations and took the form of strikes, demonstrations, electoral rallies, and public meetings.

Tilly views his book as an example of history from below. "The record of popular contention," he writes, "provides us with one of our surer guides to the experiences of ordinary people who faced great changes" (p. 386). He admits, however, that the record short-changes the inarticulate bottom levels of society, whose members were difficult to mobilize. If their experience were known, Tilly argues, it would only reinforce his view that "the growth of the state and the development of capitalism occurred at the expense of ordinary people" (p. 387). Probably so. Yet one might argue that the more efficiently organized state and the modern economy brought benefits as well as burdens to ordinary Frenchmen. Protest, though usually understandable and justified, could also be motivated by short-sighted self-interest.

Tilly has chosen to focus on five regions of France as test cases for his thesis, and he readily admits that their variations complicate the task of finding a common pattern. Managing these variations and carrying the survey through so many centuries have forced him into great structural complexity and a good deal of repetitiousness. Even the colorful accounts of crowd action eventually leave one with a sense of *déjà vu*. At the same time, some major episodes get only sketchy treatment (for example, June 1848, the Paris Commune, and the OAS sedition during the Algerian war), whereas others do not fit smoothly into the general argument (notably, the May 1968 upheaval and, in some ways, the great revolution itself). Despite Tilly's fluent style, some readers may find the book a test of endurance. But, as always with Tilly's work, reading him is well worth the effort.

GORDON WRIGHT
Stanford University

ELLERY SCHALK. *From Valor to Pedigree: Ideas of Nobility in France in the Sixteenth and Seventeenth Centuries*. Princeton: Princeton University Press. 1986. Pp. xvii, 242. $35.00.

In an informal, almost chatty, analysis Ellery Schalk evokes the hoary debates of the 1960s about crises and changes in the meaning of *noblesse* in the *ancien régime*. The earlier sixteenth-century sources stress the importance of *vertu*, or military courage, as a characteristic of nobility expressed in the profession of arms. The influence of ancient writers, the social and political upheavals of the wars of religion, and consequent debates about nobility prompted an emphasis on birth as the principal feature in descriptions of nobility of 1600. The ideas about nobility grounded on *race* are seen as complementary to the rise of the absolutist state, as is, in a sense, the increase in duelling between nobles. Schalk asserts that the military role of the nobility was in steady decline in the seventeenth and eighteenth centuries. The emphasis on education and on cultural graces—coming from outside of France—carried the nobility still further away from *vertu*.

The author draws on a wide variety of moral philosophical treatises and memoirs to try to capture the subtleties of meaning in *noblesse*. The search for social meanings almost impedes understanding of meanings of nobility as individual qualities of mind and matter. Telling analogies, such as the one made between nobility and cream, drawn by La Perrière, provide interesting clues to the perceived relationships between natural phenomena and what we in the twentieth century call social classes. For what cheese could be made from a milk (society) from which the cream had been removed? (p. 59).

Quotations from the sources provide considerable evidence that some authors employed formal philosophical arguments in the definition and use of a special vocabulary consisting of such terms as quality, action, virtue, virtues, profession, vocation, merit, honesty, and nobility. These were the "buzz words" of moral reflection in the sixteenth century, and, although the author is aware of this, he queries

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instead whether the writers were noble themselves, or more or less pronoble, but does not analyze their epistemological assumptions and specific philosophical affiliations with other thinkers.

Schalk posits that sixteenth-century writers, relatively speaking, lacked "interest and understanding" of the subject of nobility (p. 52). Writers are described as "clear-sighted" when they seem to support the author's argument (p. 99). The ancient Greeks, whose discussions of birth, virtue, and nobility are briefly summed up, are described as "inchoate" (p. 39). The writers whom the author studies are almost castigated because their thought is set in moral and theological frameworks, and he prefers those who are "less moralistic and more realistic" (p. 81). Schalk also makes some disconcerting assumptions about the role of ideas in society. Was there some "reality" outside of language to which nobles had to adapt? Ideas seem to have been a drag on nobles and have prevented them from realizing their true status. At other points, when debates about virtue seem to the author to have turned against nobles, ideas are then described as "dangerous" (p. 84).

The agenda for further research and reflection on the meanings of nobility prompted by this work can be summed up briefly by examining the extent to which philosophical discussion in the sixteenth century remained self-contained and to what extent it responded to religious and political conflict. And what an interesting agenda it is, because the individuals who participated in the discussions over *noblesse* brought a variety of experiences in politics, war, the seminary, business, and even the university to their reading and writing.

OREST RANUM
Johns Hopkins University

C. J. BETTS. *Early Deism in France: From the So-Called "déistes" of Lyon (1564) to Voltaire's "Lettres philosophiques" (1734)*. (Archives internationales d'histoire des idees, number 104.) Boston: Martinus Nijhoff. 1984. Pp. xiii, 309. $53.50.

Usually French thought from the Renaissance to the Enlightenment is considered to have been permeated with antireligious views that began as deism in the sixteenth century and culminated as atheism in the eighteenth century. Standard scholarly works by H. Busson, J. B. Charbonnel, G. Lanson, R. Pintard, and J. S. Spink have espoused this view. It is based partly on claims by horrified opponents that deism and atheism were rampant in France.

C. J. Betts carefully examines the evidence about the existence of this current from 1564 onward in terms of "deism," meaning belief in God alone. He shows that this could apply to those who believed in God but rejected the doctrine of the Trinity, the divinity of Jesus, or any organized religious view. In this sense Betts contends that no substantial evidence demonstrates the espousal of deism in France before the end of the seventeenth century and that deism did not become a full-fledged doctrine until the eighteenth century. The early allegations of deism do not reveal any serious movement, other than critiques of Christian doctrines. Admittedly, views such as some of those stated in Jean Bodin's *Colloquium Heptaplomares* could develop into a deist movement, but those crying out against deism do not reveal an existent opponent.

The opponent, a deist who believed only in God and either ignored or rejected historical features of organized religions, appeared in France when natural religion was advocated and Christianity and Judaism were interpreted as historical impostures. Betts examines alleged case after alleged case and sees J. P. Marana's *L'Espion Turc*, the travel literature of the early eighteenth century, and the clandestine literature that proliferated at that time as turning points. His case is strong if one keeps to the limits of the inquiry as stated in the title—early deism in France, an almost nonexistent phenomenon before 1700 and a flourishing one thereafter.

Part of what led to the flourishing, however, was a literature that circulated in France, but was written elsewhere, consisting of works such as Jewish attacks on Christianity, written mainly in Holland; deist literature in French written by Dutch, English, and French Protestants; and the French translation of Benedict Spinoza's *Tractatus* of 1678 as well as Spinoza's *Ethics*, which were well known in France and presented the full deist theory. French periodicals, published in Holland, discussed much of this literature. *L'Esprit de M. Spinoza, ou Les Trois Imposteurs* and Bodin's dialogues circulated widely. Copies of Bodin's dialogues were readily available in Paris around 1650, and themes that appear in *Les Trois Imposteurs* were often discussed in the second half of the seventeenth century. Isaac La Peyrère's critique of the Bible and his preadamism were well known, and J. P. Marana's advocacy of preadamism and an "islamic deism" was reprinted often. English deists (who include the author or authors of the last six volumes of *L'Espion Turc*) did package much of this into a deistic, anti-Christian view, as did some of Spinoza's followers in Holland.

So, deism in France may have bloomed later but was, contrary to Betts's picture, in part infused by currents in Holland and England and by underground literature in France. La Peyrère showed his manuscript to people in France from 1641 to 1655. Some of these people had read Bodin and were party to the early discussions of the imposture theory. The early versions of *Les Trois Imposteurs* contain much material from Pierre Charron and

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Gabriel Naudé, and material from Hobbes's *Leviathan* appears in all versions. Thus, I think a heady mix of "deist ideas" was all around and available in France, although much of the material was not written there. The change in French literature around 1700 and the philosophical expression of deist views in Montesquieu and Voltaire cannot be easily isolated from the larger story.

Betts's book is, nonetheless, a good counter to the mythology about the long and august history of French irreligion, and Betts documents that the mythology is based on rumors and overstated charges of religious opponents. One hopes this work will serve as a much-needed antidote.

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CLAUDE LABROSSE and PIERRE RETAT. *L'Instrument Périodique: La fonction de la presse au XVIII[e] siècle.* Lyon: Presses Universitaires de Lyon. 1985. Pp. 178. 90 fr.

Both in France and elsewhere, literary scholars rather than historians have long dominated the study of eighteenth-century periodicals, and this small volume includes two further significant contributions. Both essayists seek to understand the language of the press within the traditions of literary scholarship. But the similarities of approach end there.

In a relatively short article Pierre Rétat describes the rhetoric of a single newspaper, the radical *Révolutions de Paris*. Rétat has written extensively on the eighteenth-century press, and in this essay he tries to define the ideological structure of one of the most important and lively papers of the early days of the revolution. In a careful analysis of its political ideas Rétat shows how the editors of the paper emphasized the moment, sought to paint word pictures, and, to act as pedagogues and rally support for the revolution, used an impersonal form of address. Most importantly, they sought to develop an image of the revolution. By eschewing such traditional titles for newspapers as "Courier" or "Gazette," they identified their paper with the events of the day and cast the revolution in the most favorable light. Rétat's imaginative study of ideas and their presentation greatly expands our knowledge of the revolutionary press and will find a receptive audience among historians.

In contrast, Claude Labrosse's extended essay proves a complex explication of the architecture of the language of the Old Regime's literary press. Labrosse's intricate argument concentrates on four general questions. After a few introductory pages about the eighteenth-century propensity for scientific classification, he turns to his second topic: how the press clarified the reality it faced. Labrosse suggests that the press became a substitute for the clergy by interposing itself between the source of information and the public. Periodicals divided their communications into various categories and, by giving information sequentially, instilled a sense of change. With a still more narrow focus Labrosse then asserts that literary journals acted as a filter for a wide public through their selection of materials for publication. Their summaries and reviews of books preserved, collected, and circulated materials. For this section, Labrosse read few periodicals but instead consulted contemporary definitions of the proper role of the medium.

The last half of Labrosse's essay concentrates on how literary magazines interpreted the novel in 1761. He shows that reviews emphasized plots, pleasant and imaginative passages, and moral statements. Labrosse then leaps toward provocative conclusions. By weaving together morality and escapism, periodicals reinforced the social fabric because they emphasized correct behavior and gave readers an imaginary outlet for their passions. More boldly and more significantly, Labrosse maintains that, by promoting conservative values, periodicals both gained acceptance and separated reader and author. For Labrosse this division was crucial in depersonalizing the relationship between writer and audience and was the first fundamental step in the creation of the modern, anonymous public. Concentrating on the basic rhythms of the periodicals' language allows Labrosse to link the content of the press to the public.

Still, historians will be uncomfortable with much of Labrosse's analysis because of his focus on linguistic structures and his consequent failure to explore the more mundane elements of the message. More importantly, historians will find Labrosse far more interested in speculations than sources. Throughout the work a cavalier attitude toward evidence allows facile connections between information and conclusions. This indifference to precision is reflected in the structure of the essay, which implies that the tendency to classify largely explains the emergence and broad acceptance of the press. Many scholars will challenge such a vague, even metaphysical, explanation and insist on discussions of material conditions, intellectual developments, and state intervention. Finally, jargon and repetitive arguments mar Labrosse's essay.

Such failings notwithstanding, this book makes an important contribution that historians ought to contemplate. In their struggles to propose approaches for evaluating public and popular *mentalités*, scholars have begun to emphasize the significance of reading. Labrosse makes a most interesting, if inconclusive, addition to this field by explaining how public

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attitudes may be fathomed. He convincingly posits that the basic linguistic usages in the press had to influence and relate to readers' tastes. The analysis of such texts can provide clues to the public's understanding. More prosaic interpretations than Labrosse's indeed may elucidate only arguments rather than shared views. Labrosse himself has given proof of the efficacy of his approach in his *Lire au XVIII^e siècle:* La Nouvelle Héloise *et ses lecteurs* (1985). Other scholarly efforts will be necessary to validate this method of analysis.

JACK R. CENSER
George Mason University

JOHN M. MERRIMAN. *The Red City: Limoges and the French Nineteenth Century*. New York: Oxford University Press. 1985. Pp. xix, 332. $29.95.

John M. Merriman's book recounts the history of Limoges from the Restoration to World War I. The story is interesting, perhaps even exemplary. Something of a backwater at the beginning of the nineteenth century, Limoges became a major industrial center, specializing above all in porcelain, secondarily in boots and shoes. Rapid industrial development powered a major shift in political orientation, from fervent Catholicism and monarchism under the Restoration to no-less-fervent socialism and anticlericalism in the Belle Epoque. The stages in this political evolution were marked by the familiar revolutions. The July revolution effected a clear shift from clerical and noble domination to that of a determined and self-confident commercial and industrial bourgeoisie. The revolution of 1848 (treated rather cursorily in Merriman's account) witnessed the emergence of a significant working-class political presence, which was reasserted during the Commune and attained its apotheosis during the socialist municipality of 1895–1906. The quasi-insurrectionary strikes of 1905 confirmed Limoges's reputation as "the red city" but also marked the high point of working-class militancy. Thereafter, Merriman states in his epilogue, the workers of Limoges continued to vote for the Left but learned to play by the rules of the capitalist game and sought higher wages and reforms rather than the establishment of a socialist society.

Merriman's account is structured by political events, but it is not merely a political history. He writes at some length about changes in the urban landscape, in popular culture, in public celebrations, and in the scale and technology of production. He has an impressive mastery of the archival sources and a fine eye for the significant detail and illuminating anecdote. His three chapters on the period from 1890 to 1905, which make up nearly half the book, are particularly rich. But, although the book is dedicated to Charles Tilly, its real affinities appear to lie with Richard Cobb or perhaps with the "new" Lawrence Stone: this work is "revival of narrative" with a vengeance. Numerical tables are few and simple. We never get an exact picture of the changing composition of the labor force or of insurgent crowds. Changes in technology and work experience and their influence on workers' movements are not systematically examined. Merriman makes little attempt to compare developments in Limoges to what is known about other French cities. He asserts on various occasions that political changes in Limoges "resulted from the economic and social transformation of the city" (p. 43). But the causal links are never specified, argued, or subjected to systematic tests; socioeconomic determinism serves merely as an unexamined backdrop for the narrative. Readers of this book will be charmed by its evocative and engaging style and will learn a great deal about politics and society in Limoges. But they may also be reminded of why social historians of the 1960s and 1970s revolted against purely narrative history in the first place.

WILLIAM H. SEWELL, JR.
University of Michigan

RAINER RIEMENSCHNEIDER. *Dezentralisation und Regionalismus in Frankreich um die Mitte des 19. Jahrhunderts: Politische Bewegungen gegen den Verwaltungszentralismus im Umkreis von Februarrevolution 1848 und napoleonischer Restauration 1851*. (Pariser historische Studien, number 22.) Bonn: Ludwig Röhrscheid. 1985. Pp. 301.

This is a thoroughly researched, minutely detailed, painstakingly argued book about something that did not happen between the February revolution of 1848 and the Bonapartist coup of December 2, 1851: France's political life was not decentralized, despite what Rainer Riemenschneider insists was an emergent regionalist consciousness.

Perhaps this is not so surprising. Given the general discrepancy between the good intentions of the lawgivers of the Second Republic and their derisory achievements, their inability to apply the democratic principle they had endorsed by proclaiming universal male suffrage to create a system of genuine local self-government was predictable. Despite the ritual complaints so familiar in French history of the stifling of provincial life by Parisian control, practical measures to limit centralization have usually been few and feeble. So they proved from 1848 to 1851, although the author devotes a full chapter to each of these mostly abortive measures and thoroughly analyzes the parliamentary debates and editorials that constitute his essential sources.

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The Constituent Assembly did enact a "provisional" law providing for the election from and by municipal councils of mayors of less-populated communes, who hitherto had been appointed from Paris. But this measure was decentralization's one, short-lived success. The assembly rejected the basic concept of local self-government, a notion, as contemporary Germans pointed out, that French could not express without circumlocution. The later Legislative Assembly took no account of the demands, after the June days, of many *conseils-généraux* that they be empowered to govern France in any similar emergency.

Riemenschneider insists that, contrary to general belief, the motive for proposals to decentralize was not always conservative provincial fear of radical Paris. He devotes a chapter to the curious bill proposed by two deputies of the far Left aiming to reorganize France on the basis of cantonal-level self-government. Since these cantonal governments would be responsible for maintaining official warehouses of foodstuffs and for making inventories of local property ownership—"Précurseur du partage!" interrupted one angry conservative—the legislature's rejection of such embryonic *phalanstères* by a margin of 515 to 62 (with 23 abstentions) is not surprising.

If his investigation is sometimes excessively detailed, the author cannot be faulted for lack of analytical curiosity. He shows, for example, that much of the support for this radical proposal came from the relatively few deputies of lower-class origins. Middle-class deputies, even on the Left, he explains, were already integrated into France's existing political order and saw decentralization as a prescription for national dissolution.

Riemenschneider sometimes carries this social analysis of deputies' varying views of decentralization farther than some readers will be willing to follow. His most convincing chapter shows geographical as well as social forces at work: much conservative support for decentralization came from former *pays d'état* that were annexed late, such as Brittany. He even ventures that this support reflects an early stage of the cultural particularism that has come to full bloom with the regionalism of the 1980s.

Noting that, even after the Socialists' reforms of 1982, France remains the most centralized of modern nations, the author implies that 1848 was the turning point at which the history of French local self-government—and thus of French democracy itself—failed to turn. Yet need we really ascribe the Second Republic's perpetuation of centralized government, as the author does, to the continuing political dominance of an urban, industrializing, modernizing, Orleanist-inclined bourgeoisie who feared the enfranchisement of the rural masses, whether "white" or "red?" If Eugen Weber is right in arguing that the France of 1848 was more like the France of 1789 (and, indeed, 1793) than the France of 1889, the reasons for the failure of the impulse to decentralize are not far to seek. Perhaps politicians of all stripes recognized in 1848 that "France" was still something of an artificial construction, held together only by the administrative armature in which Napoleon had encased its innumerable *pays*. From such a perspective decentralization implied disintegration.

JOHN ROTHNEY
Ohio State University

STANLEY L. JAKI. *Uneasy Genius: The Life and Work of Pierre Duhem.* (International Archives of the History of Ideas, number 100.) Boston: Martinus Nijhoff. 1984. Pp. xii, 472.

Pierre Duhem's (1861–1916) fame depends on his work in three areas: thermodynamics, the history of science, and the philosophy of science. The eponymous Gibbs-Duhem relation celebrates his achievement in the first area; in the second he is credited with "discovering" the Middle Ages and their importance for the Copernican scientific revolution. His philosophic output—he was a Roman Catholic positivist—brackets him with his contemporary, Ernst Mach. Yet despite such a legacy, little is known of Duhem, save that he was French and for hidden reasons never achieved adequate recognition from the French intellectual establishment, for he spent his life teaching and researching in provincial universities. Stanley L. Jaki's book, based on archival and manuscript sources as well as on Duhem's vast publications, attempts to remedy that deficiency.

It is actually two books: the first section primarily offers a narrative of Duhem's education and of his teaching career; the second part consists of three long chapters, one each on Duhem's work in the areas on which his fame depends. These chapters end with a discussion of the impact of Duhem's work on his contemporaries and subsequent critics. Biographers are drawn to their subjects for a host of reasons. Jaki is doubtless Duhem's prime apologist: he introduces the second half of the book with the statement that it is "in a sense an argument on behalf of (Duhem's) . . . views" (p. 246). Such a split in a biography may indeed have been necessary in view of the vast amount of available data that Jaki rightly felt compelled to include. On the other hand the reader is confronted with what is now an uncomfortable Victorian assumption that "work" is in some way divorced from and independent of "life." In Duhem's case that assumption is obviously incorrect, as, indeed, it must be for anyone.

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In the first half of the book Jaki comes to grips with Duhem's virtual isolation in provincial universities. He sees it as arising from a complex interplay between Duhem's personality, his Roman Catholic point of view, and his outspoken criticism of the powerful P. E. M. Berthelot, who was similarly engaged in the history of science (in his case, the history of chemistry), in chemical research, and in the philosophy of science. To superficial eyes the French scientific community appears monolithic and united; closer examination, which Jaki's exposition of the Duhem case enables, shows that it operates with the same kinds of political maneuverings that occur in countries lacking an Académie. Duhem's case offers another instance of politics in high places reverberating in intellectual matters.

Sociological insights would have enriched Jaki's large study, but they would have extended the work even more. For now readers will be content to mine the archives on which Jaki's biography depends. Synthesis and close analysis are still to come.

J. Z. FULLMER
Ohio State University

JEAN-NOËL LUC. *La statistique de l'enseignement primaire 19e–20e siècles: Politique et mode d'emploi.* (L'ecole à travers ses statistiques.) Paris: Economica or Institut National de Recherche Pédagogique, Paris. 1985. Pp. vii, 242. 110 fr.

A book on the history of nineteenth- and twentieth-century French primary education statistics is unlikely to attract a large number of American historians. Jean-Noël Luc's volume, however, should be required reading for all who do research involving French educational statistics, and it is highly recommended for anyone wishing to assess the accuracy of generalizations about the magnitude of French primary schooling in a given year or its expansion from one political regime to another.

The Institut National de Recherche Pédagogique's publication of this and other volumes indispensable for researchers is testimony to the vitality of the study of the history of education in France. This field has been invigorated recently by social historians interested in such topics as the relationship between schooling and social class and the disparities between the education ministry's national policy directives and educational realities at the local level. Information about the numbers of schools or pupils is essential for such inquiries, and researchers need to know about the reliability of familiar sets of published figures. To explain why some statistics are problematical, Luc first chronicles the political and policy-oriented concerns determining the gathering and presentation of statistics during the nineteenth century, the declining interest in acquiring educational statistics between 1910 and 1956, and the statistical explosion since the late 1950s, which coincided with an unprecedented expansion of secondary schools.

Like some other recent works—most notably François Furet's and Jacques Ozouf's *Lire et écrire* (1977)—this volume has the revisionist intent of debunking some of the Third Republic's claims for educational innovation, particularly with regard to concern about instructing the masses. Luc demonstrates that republican reformers underestimated their predecessors' interest in popular education because they looked primarily at previously published statistics and ignored various unpublished *enquêtes* ordered by education ministries. Luc's revisionism, however, is carefully nuanced, and he even deflates some revisionists' claims, for he notes that certain figures from before 1870 artificially inflated the numbers of primary pupils. Luc's statistical revisions are of two sorts: first, the reduction of annual enrollment figures that were exaggerated between 1859 and 1914 because of imprecise methods of counting pupils and, second, adjustments made for annexation of the territory of Nice and Savoy in 1860 and the loss of Alsace-Lorraine in 1871. To illustrate the magnitude of Luc's major statistical revisions, three examples must suffice. By his reckoning, the increase in primary school enrollment from 1850 to 1866 (typically attributed to the "authoritarian" Second Empire before the law of 1867 sponsored by the liberal education minister, Victor Duruy) should be reduced from 36 percent to 14.7 percent. The increase between 1866 and 1881, now recalculated, rises from 18.3 percent to 30.5 percent. Although these corrections do not upset the basic generalization from other studies that most children received some schooling before Jules Ferry's compulsory education law of 1882, Luc disagrees that 96 percent of six- to thirteen-year-olds were already schooled in 1863. Instead, he offers the figures of 86 percent for 1876 and 96.5 percent for 1881 (albeit with the reservation that these are also overestimations).

Historians of education will appreciate Luc's identification of unpublished statistical sources in the Archives Nationales and selected departmental archives, and they can also look forward to a planned companion volume offering revised sets of statistics.

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MARILYN R. BROWN. *Gypsies and Other Bohemians: The Myth of the Artist in Nineteenth-Century France.* (Studies in the Fine Arts, the Avant-Garde, number 51.) Ann Arbor: UMI Research Press. 1985. Pp. xv, 229.

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Gypsies and other social outsiders loosely called bohemians fascinated many in nineteenth-century France partly because so little was known and so much was imagined about them. Marilyn R. Brown ably shows what was imagined by artists and offers a social explanation of it. Throughout the book Brown refers to eighty-five black-and-white illustrations grouped at the back, but she also considers the presentation of bohemians in novels, plays, poetry, and music. Indeed, she has drawn on and assiduously researched a full gamut of the sources and theory of social history. Her purpose is to "reevaluate the modernist myth of bohemianism" (p. 2), a system of beliefs about unconventional artists not only as an avant-garde but also as a marginal group in some ways similar to gypsies, beggars, and street performers.

Like earlier historians of the subject, Brown asserts that the bohemian myth emerged under the strikingly bourgeois conditions of the July Monarchy. Salon artists began producing numerous paintings of exotic wanderers, while such printmakers as Victor Adam created sympathetic lithographs of urban vagabonds, and Honoré Daumier made satirical prints of street people, representing bourgeois charlatans. Bohemians were an "other" who could be perceived as remote and unchallenging or nearby and troubling. Artists who took such varied views were just as divided in their attitudes toward their own social position. Faced with the loss of traditional patrons, artists wrestled with the insecurities of the marketplace and the temptation of "selling out" to new art buyers. In depicting bohemians, they sketched their own alternatives—the happily independent and mobile artist, the wretched outcast, or the wise, truly superior social rebel.

Brown judges this art by the degree to which it showed social realities. Her judgments of works after 1851 are overwhelmingly negative. As gypsies and street people faced repression and greater hardship, most artists treating bohemian subjects stuck to fantasy and self-reference. Brown finds that the highest number of paintings of gypsies were exhibited in the 1860s. But by then, she notes, the bohemian had become an artistic stereotype, a "bourgeois myth," perpetuated by artists less concerned with the victims of society and more absorbed in their own quest for identity and success. Artists of the establishment as well as the avant-garde thus comfortably treated bohemians. The book is strong in its illumination of the complexities of mythmaking and social change, in place of simpler schemas, such as a bourgeois-versus-bohemian opposition.

Although artistic bohemia was repeatedly declared dead, new generations of artists resurrected a postured bohemia both in their life style and in their works. The myth, which at times had celebrated social conflict and change, ultimately reconciled oppositions and confirmed the existing social order. Brown cites Picasso's paintings of *saltimbanques* as a concluding example. In his new book on bohemian Paris, Jerrold Seigel takes the more optimistic view that bohemia opened up new possibilities for art and bourgeois experience. Brown's book, with its fuller treatment of the visual arts and its darker interpretation, is a worthy complement and counterpoint to Seigel's.

CHARLES REARICK
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CHARLES REARICK. *Pleasures of the Belle Epoque: Entertainment and Festivity in Turn-of-the-Century France.* New Haven: Yale University Press. 1985. Pp. xiii, 239. $29.95.

JERROLD SEIGEL. *Bohemian Paris: Culture, Politics, and the Boundaries of Bourgeois Life, 1830–1930.* (Elisabeth Sifton Books.) New York: Viking. 1986. Pp. ix, 453. $25.00.

Although they do so in vastly different ways, both these handsomely produced books seek to illuminate part of the landscape of modern French culture. Those occasions where their subjects coincide, as in the "bohemian" Montmartre of the *fin-de-siècle*, serve to underline their distinct orientations. In his lavishly illustrated book Charles Rearick has written a social history of popular culture from 1870 to 1914, emphasizing festivals and fairs, the capital's new theatrical spectacles, and a host of even more informal entertainments of the new democratic age. Jerrold Seigel's study treats the apparently restricted subject of the Parisian artistic and literary underworld from 1830 to 1930, but it is a more ambitious (and much longer) book than Rearick's and gathers popular culture, political theory, and the hermetic treasures of French high culture under the same rubric. The book with the simpler agenda—Rearick's—is full of lively speculation, unresolved contradictions, and moral fervor, whereas Seigel has woven a multiplicity of threads into a single fabric with analytical detachment. Rearick's study turns up new facts, whereas Seigel explores well-known turf. But Seigel's is by far the better book: informed, thorough, discerning in judgment, and sensitive to nuance—an interpretive triumph.

The points of agreement in these books are interesting. First, neither Seigel nor Rearick are taken in by the myth that artists and their growing public were somehow happier or more fulfilled in an era that demanded and received the sacred right to enjoyment. Both amply illustrate the melancholy aspects of the production and consumption of culture in bourgeois society. Rearick persuasively, at

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times poignantly, shows that entertainments became less spontaneous, less intimate, and less participatory and sought to attain increasingly more intense levels of physical and psychological stimulation. He makes his case by discussing the official creation of Bastille Day in 1880, the progressively larger World Fairs of 1878, 1889, and 1900, and popular theater in the capital. Although he cannot demonstrate decisively that the masses experienced the new entertainments less meaningfully than they had traditional festivities, he does provide proof that many of the creators or early enthusiasts of mass entertainment came to doubt, or even dread, its effects.

For his part, Seigel effectively demolishes the stubborn legend of a carefree bohemia in which independent spirits found the liberty to create outside the suffocating disciplines of bourgeois life. From Henry Murger to André Breton, he argues, the conditions of bohemian existence—poverty, social and legal marginality, powerlessness—provoked in those who lived there the need to dramatize their ambivalence to the ordered middle-class life they could not or would not join. This ambivalence expressed itself in their lives and in their work as a series of exaggerations of the bourgeois qualities they simultaneously craved and feared: the cult of the self became dandyism, slovenliness, or the adoption of eccentric persona; bourgeois egotism became unabashed self-advertisement and opportunism; bourgeois self-indulgence became drunkenness and drug addiction; bourgeois virtue was transformed into puritanical revolutionary zeal or obsessively self-disciplined work regimes. The successful bohemian sank neither too low, into crime or incoherence, nor rose too high, into emasculating notoriety, but used marginal existence creatively to test the limits of human endurance and the boundaries of the human estate. But even the greatest and most visionary residents of Seigel's bohemia—Charles Baudelaire, Arthur Rimbaud, Jules Vallès, Alfred Jarry—could not sustain its rigors without escape, prevarication, or solipsism.

Both Seigel and Rearick lay bare the chains that bound artists and entertainers to a capitalist culture they often vilified, but which provided their bread and published their work. Rearick shows that café-concerts and cabarets provided opportunities for marginal artists to display their talents or work but rewarded most generously those who employed the most effective formulas and adapted best to shifting tastes. Seigel writes of the would-be poets, who dreamed of artistic integrity but hired out their pens to the little newspapers or scandal sheets of the 1840s and 1850s, and of the cultivated eccentricities of turn-of-the-century painters, whose nonsense and innovations now attracted a reliable consuming public.

As beautifully as he evokes the pathos of modern mass culture, Rearick does not succeed in explaining it. He is content to describe how modern entertainments, of which he disapproves, displaced traditional festivals, for which he evinces much nostalgia, and concludes that something irreplaceable has been lost. He employs certain key words—"intimacy," "spontaneity," "play"—in place of a systematic (anthropological or historical) definition of disappearing traditions and then turns them against the mechanical, soulless culture he dislikes. He appears to believe that modernization swept away the old forms of popular culture, but much of the evidence he provides suggests that, if the forms of culture changed to incorporate urban audiences and new technology, the contents often remained the same. The villain in his tale is the "government," a faceless, moralizing monolith that tried to create modern festivals in place of the old ones, failed, and left "ennui" and alienation in its wake. This attitude is a poor substitute for the patient investigation of the aims of the politicians and administrators who organized national festivals and fairs, of the political climate within which they worked, and of the material and ideological limitations that may have informed their efforts (see on this issue the essays in Eric Hobsbawm and Terence Ranger, *The Invention of Tradition* [1983]).

Seigel's book may be thin on the social and economic features of modern culture, which is the strength of Rearick's book, but it compensates for that by the range and subtlety of its analysis. As must be clear by now, Seigel's bohemia was not only a place but a mode of being, a refuge, a set of temptations, and a creative space in which many of the most brilliant artists, writers, and musicians of modern France lived and worked. Their very marginality was both the source of the tensions that fueled their inspiration and the basis of bourgeois fascination with bohemia and its cultural myths. The ingenuity of Seigel's accomplishment is to make bohemian and bourgeois into a dialectical couple, conferring endlessly on the discreet charms and vulgar defects of middle-class life.

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JEAN-MARIE MAYEUR. *Catholicisme social et démocratie chrétienne: Principes romains, expériences françaises.* (Histoire.) Paris: Cerf. 1986. Pp. 287. 129 fr.

The appearance of this book is an additional fine example of the thorough and challenging scholarship of Jean-Marie Mayeur. Trained in the rigor and boldness of a burgeoning French school of religious and sociological thought, Mayeur reflects

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the superlative skills he has learned from his mentor, Emile Poulat. Although this collection of numerous articles covers a wide range of topics, the author never strays from his avowed purpose for the entire book: "The works brought together in this volume have no other ambition save to clarify by convergent readings a similar and great history" (p. 14). Indeed, Mayeur has accomplished this task, but the greatness of the book lies more specifically along two lines.

First of all Mayeur has significantly added to the store of knowledge in the exploding field of social Catholicism. In this he does honor to the pioneering work of others such as Adrien Dansette, René Rémond, and especially Poulat. The book is divided neatly into topics of definitional concerns, of comparative international Catholic movements, and of matters dealing with exclusively French concerns. One is struck by Mayeur's breadth of scholarship and precise insights as he leads the reader from topic to topic. His article on the origins of Leo XIII's social teaching effectively highlights the impact of Thomistic theology on the church's social philosophy. Not only does Mayeur develop the major themes of *Rerum Novarum* he also sets this pioneer encyclical in its global context. All the central tenets of the evolution of social Catholicism—its dedication to corporatism, its commitment to a reactionary and conservative world view, and its slow incorporation of liberal ideas—are contained in this synoptic article and can be used as a guide for subsequent chapters. Mayeur's study on Pius XII's relation to European Catholic movements, especially the Christian Democratic parties, is a finely nuanced piece, and the brief look at the influence of social Catholicism since *Quadragesimo anno* is a most convincing analysis.

Mayeur's articles dealing exclusively with the French scene are, in a number of cases, significant additions to an already expanding field. The works on Père d'Alzon, the Franciscans, and the ties of social Catholicism to middle-class organizations are valuable contributions, and the study of Charles de Gaulle's relationship to both Catholic social thought and its antithesis, statism, is brilliant in its analysis. Mayeur's lengthy study of the three Christian Democratic congresses in the late 1890s demonstrates a unique blend of descriptive thoroughness and interpretive skills.

In spite of all these important additions to scholarship, however, the chief merit of Mayeur's book lies in a second area. The work, especially as indicated in the first chapter, raises with utmost clarity the definitional issues in the field of social Catholicism. He links together the sometimes common, sometimes contradictory threads of the social Catholic tradition from the nineteenth century to the postwar Christian Democratic explosion. He convincingly asserts that the growing elements of social democracy within Catholic social thought find their roots chiefly in the reactionary and conservative protest of populist Catholics against the political and economic liberalism of the nineteenth century. This serves as a nuanced corrective to a too-facile identification of Christian Democrats as leftist political figures. Finally, he lays the base for understanding the leftward drift of social Catholics in terms of the infusion of politically liberal ideas into a system historically rooted in social conservatism. Although one must challenge Mayeur to give more credence and attention to an evolving radical strain in social Catholicism, historians of the field owe him a debt of gratitude for this masterful work.

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JULIAN JACKSON. *The Politics of Depression in France, 1932–1936*. New York: Cambridge University Press. 1985. Pp. x, 303. $39.50.

The familiar litany of criticism of French political leaders of the 1930s is lengthy, sharp, and often bitter: politicians refused to renew republican institutions, or even to collaborate in the national interest, while societal dislocations became pronounced, and Hitler was permitted, owing to French vacillation and weakness, to dismember the European world. In sum, French leaders allegedly left everything unattended, even as their political system unraveled under multiple pressures, domestic and foreign. And central to this litany is their dismal response to the depression, which hit France later than other industrial nations but persisted much longer, sapping the country's vitality and consequently its ability, even its will, to resist the aggressive actions of Nazi Germany. In this excellent, carefully documented account of events, parties, and personalities from 1932 through to the election of 1936, Julian Jackson explores the politics of dealing with the depression and the policies, or absence of policies, to combat it; he also gives a fine summary analysis of the 1936–39 period. Clearly, politicians of nearly every persuasion were afflicted with near-total ignorance of economic thinking. More important, however, was the pleasure they took from their ignorance, for it permitted them to look almost exclusively to political solutions to economic and financial problems. Their proposed solutions, inevitably driven by political calculations, cancelled out each other, with economic stagnation the only sure result.

Since parties, such as they were, represented fairly well defined slices of society, they spoke for

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particular interests, always potentially in conflict. Within the groups on Left and Right deep divisions existed on economic and financial, as well as political, matters. In the main, the Left reacted to the depression by exercising political opportunism and resisting those in its ranks who advocated restructuring the economy through a "plan." On the Right partisans of economic liberalism were dismayed to discover that a policy of deflation required increasing action by the state, and the fragility of the franc and its possible devaluation, and budgetary disequilibrium, distracted many from the central matter, the depressed economy.

Jackson expertly threads his way through the morass of failed policies and missed opportunities and suggests that few politicians had any confidence in the regenerative possibilities of capitalism. By inclination and perhaps by design, governments opted for economic stagnation as the only way out of the depression. Even the Popular Front, he argues, was mainly a traditional republican movement, guided by politics and the search for political advantage. Rather than sparking economic revival, it served to restore the traditional political system; it was part of a familiar rhythm of Third Republican politics.

Surely, certain of Jackson's conclusions and points of emphasis may be debated, especially his views of the enthusiasts for a "plan" and the Popular Front. Yet his very rewarding study is a fresh approach to understanding of a troubled era.

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REINHARD KAPFERER. *Charles de Gaulle: Umrisse einer politischen Biographie*. Stuttgart: Deutsche. 1985. Pp. 319.

Few contemporary statesmen have had as many biographies written about them as Charles de Gaulle. At present, all three volumes planned by Jean Lacouture have been published; their two thousand four hundred pages amount to thirty pages per year of the general's life. Reinhard Kapferer, a German jurist and political scientist, covers all of de Gaulle's life in two hundred seventy pages (plus thirty pages of excerpts from the general's writings). Obviously, the reader is not likely to find any revelations here. Kapferer primarily uses de Gaulle's works and a small number of secondary works (one of my few criticisms concerns his excessive reliance on the many volumes of a highly anecdotal French journalist, Raymond Tournoux, whose stories and quotes are not always beyond doubt).

Kapferer's point of view is most interesting. He is a German, dealing with a French statesman whose life, from beginning to end, can be seen as a confrontation with "the German question." Kapferer is at his subtle best when he tries to clarify de Gaulle's complex and fluctuating thoughts and feelings about France's neighbor. He shows the deep mark of Maurice Barrès's thought on de Gaulle but also the difference between de Gaulle's sense of the Franco-German antagonism and the "rabid, absurd, racist-metaphysical, anti-German arguments of the Action Française" (p. 38). De Gaulle never saw Germany as the permanent incarnation of evil, and he expressed in many of his works a remarkable capacity for empathy and admiration (his first book, *La discorde chez l'ennemi* (1924), was about Germany's performance in World War I). Kapferer also shows the continuity of de Gaulle's fears about a united German Reich and about "German uncertainties" (such as the risk of a new Rapallo); the only exception was the brief period of intense friendship and cooperation with Konrad Adenauer, an episode that ended abruptly when the Federal Republic made clear that it did not endorse de Gaulle's grand design for a "European Europe" from the Atlantic to the Urals and that it preferred America's protection to France's leadership. Kapferer correctly stresses that recent French anxieties about Germany (for instance about the effects of antinuclear movements in the Federal Republic) reflect the same misgivings as de Gaulle's. He could have emphasized just as correctly that, despite his fears and disappointments, de Gaulle gave to the Franco-German reconciliation that had begun during the Fourth Republic a solemnity and importance that have never been challenged since then.

The other main characteristic of Kapferer's book is his intelligent admiration for de Gaulle. He seems to have understood the man's character perfectly. He deals with de Gaulle's concept of politics and leadership with discrimination and unfailing fairness. The young de Gaulle sometimes sounded like a militarist, but he never doubted the need for civilian supremacy. His ideal leader was certainly not a democrat but was not a fascist or a Bonapartist either. He disliked parties and the French brand of parliamentarism, but he was no advocate of coups d'etat. His apparent "conversion" to a more leftist program during the war was not opportunistic, but a response to the needs of France that blended with his desire for unity and renewal. Above all, Kapferer does justice to de Gaulle's inflexible defense of French integrity and to his extraordinary gifts of will and leadership. This book is not hagiography. But it is certainly not by accident that all attempts at describing de Gaulle's astonishing career that are neither deliberately hostile nor displays of hero worship are overwhelmingly positive

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and full of appreciation for the human being as well as for his achievements.

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JAMES S. AMELANG. *Honored Citizens of Barcelona: Patrician Culture and Class Relations, 1490–1714.* Princeton: Princeton University Press. 1986. Pp. xxvi, 259. $30.00.

Historians have assumed that the aristocracy of early modern Spain was broadly equivalent to that of France or England—that is, a hierarchy of fief holders rising to a pinnacle in courtly titles. Medieval historians are perhaps more alive to the crucial political importance of the urban aristocracy of Spain. The enormous territorial jurisdictions of the royal towns and the struggles of lineages for control over them have their nearest parallel perhaps in the Italian communes. A great welcome must be extended therefore to James S. Amelang's study of a Spanish urban elite in the early modern period. As the last of the Iberian communes to hold out against the encroaching powers of absolute monarchy in 1640–52 and 1705–14, Barcelona deserves particular attention in this regard. Understandably, the Castilian cities, especially Seville, have attracted more attention from historians in this period of Atlantic imperialism. But they rarely achieved the economic or political maturity and sophistication of those great city-states of the Mediterranean seaboard, Barcelona and Valencia.

The key to understanding this maturity, perhaps, is the role of the honored citizens, a self-recruiting patrician elite that enjoyed many of the powers and prestige of the nobility but did not fully share nobles' emphasis on lineage. The weakness of aristocratic states, as Montesquieu warned and as the decline of the Castilian commune may illustrate, is their exclusiveness, an obstacle overcome in the case of Barcelona by de facto admission of lawyers to the privileges of the group and by increasingly generous creations of new citizens by the king. Amelang's argument is that through a concept of status that emphasized education and "virtue" rather than mere birth, the honored citizens and the old nobility of the sword fused during the early modern period into a cohesive ruling class. The education in question was a program for governors rather than an avenue of social mobility in itself. It aimed to provide not vocational training but the character building that men born to rule required: in particular, control of the passions and the ability to lead. Prestige in Mediterranean cities, in Italy as well as in Spain, seems to have come from a high public profile and from the exercise of political responsibility. Before about 1700 the socially privileged were more visible than ever since, not only as competitors for municipal office but also as actors in the widest sense, fighting bulls in the marketplace on feast days or jousting with their fellows to the applause of their servants and inferiors. The honored citizens adapted perfectly to this knightly mentality, but, argues Amelang, they contributed their own emphasis on individual talent and learning. Perhaps this contributed in the long run to the withdrawal of the Barcelona elite from participation in public festivity in favor of either an amused spectatorship (which Amelang neatly summarizes as a "retreat to the balcony") or a censorious disapproval of popular hooliganism.

Amelang's work is innovative and thought provoking. Inevitably, as he himself recognizes, our knowledge of the social history of Barcelona, and indeed of early modern Spain as a whole, suffers from enormous gaps. The present study relies heavily on an impressive range of moral treatises and pamphlets linked to grand occasions such as festivals and funerals. One wonders whether more use of wills and genealogies—in the absence of diaries or letters—would modify our concept of the successful fusion of the honored citizens and the nobility. But the great merit of Amelang's book is to redirect attention away from a simple head count as a way of establishing the shape of the social hierarchy (counting creations of new citizens or nobles is more useful for the political than the social historian, one feels at times), toward the elite's perception of itself. The honored citizens' view of themselves, or at least how they wanted others to view them, is now a lot clearer thanks to this fine study.

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HARALD JØRGENSEN. *Lokaladministrationen i danmark: Oprindelse og historisk udvikling indtil 1970; En oversigt.* [Local Administration in Denmark: Origins and Historical Development until 1970; An Overview]. (Administrationshistoriske Studier, number 11.) Copenhagen: Rigsarkivet or G. E. C. Gads. 1985. Pp. 627. 183 KR.

Denmark's local governments were drastically reorganized and consolidated in 1970, and, lest people forget what they used to be like, the Danish Humanities Research Council commissioned a team of scholars to make definitive historical studies of them. State archivist Harald Jørgensen led the team and contributed the present book as survey of the whole subject. Ten other, more specialized, studies preceded its publication.

In Denmark local administration encompasses more than strictly local governments. It extends to

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all things administered at the local level, whether on instructions of a town council or of the central government. Jørgensen, therefore, felt obliged to include in this unprecedented history descriptions of all manner of things locally administered, from tax collecting and lighthouse keeping to the upkeep of roads, schools, and churches.

The history is divided into four periods: (1) the pre-seventeenth-century era, when local order was kept by a bewildering (and poorly documented) mix of folk, feudal, and church institutions; (2) the period 1660–1848, when all administrative entities were placed under the central control of the crown; (3) the period 1848–1919, which saw an expansion of governmental functions and a modest grant of authority to locally elected officials; and (4) the period 1919–70, which saw the establishment of the modern welfare state and great enlargement of the corps of public administrators, almost all of whom were trained in their specialties and many of whom were women. This history stops in 1970, when the number of national administrative districts was reduced from 25 to 13 and the number of local communities was reduced from 1,300 to 277.

Jørgensen bases his history on official archives rather than on newspapers or personal memoirs, for which reason he offers much technical detail but tells little of politics. His fondness for old documents is evident in a final forty-page section that recounts the available official records, registries, protocols, and communications and where one can go to find them. It is handsomely illustrated with pictures of venerable bookshelves, cabinets, and bundles in which the records are kept.

These records were originally compiled by officials mindful of the crown. They awaited inspection from the top. This book has all the virtues of this orientation, but it is definitely not a source to consult when one wishes to learn how ordinary people of times past coped with the prefects, priests, clerks, and tax collectors that kings set before them.

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PAUL RICHARD BLUM, editor. *Studien zur Thematik des Todes im 16. Jahrhundert.* (Wolfenbütteler Forschungen, number 22.) Wolfenbüttel: Herzog-August Bibliothek. 1983. Pp. 198. DM 64.

With this volume the distinguished series Wolfenbütteler Forschungen enters an area of historical study not widely established in German scholarship. Death is, nevertheless, a ubiquitous theme in sixteenth-century German culture, and no justification is required for investigations into the modes in which sixteenth-century people experienced death. In the volume Paul Richard Blum brings together thoughtful and sound studies by eight participants in a *Gastseminar* at Herzog-August Bibliothek, Wolfenbüttel, in 1981. The authors employ a variety of well-founded methods and use noncontroversial sources. The articles comprise a historiographical review, an analysis of the meaning of symbol and caption in emblemata, three literary-critical analyses—of a medieval and humanistic commentary, of carnival plays, and of evangelical hymnody. Funeral sermons present a special methodological problem but yield to content-narrative analysis. Two concluding studies—one on burial ceremonies accorded undesirables or indigents, the other on ceremonial obsequies commemorating Charles V—venture into thoroughly established interpretations in social history.

The first article of the collection, by Kuno Böse, is a review of extensive recent French scholarship on death. The sixteenth century appears to be a transition from late medieval preoccupation with the Last Judgment toward the desacralization of death during the Enlightenment. Explaining this change in attitude will require more than just the historian's method; German scholars can well afford to become acquainted with their French counterparts.

Wolfgang Eckart's article, "Die Darstellung des Skeletts als Todessymbol in der Sinnbildkunst des 16. und 17. Jahrhunderts," portrays several powerful meanings for death conveyed by the skeleton: the fleeting nature of life; the immortality of the soul; life growing out of death; death as friend, enemy, or judge. Widely employed as a death image, the skeleton did not always convey a horrible meaning for that way station between lives.

The *Aeneid* of Vergil, specifically the costly error of Palinurus the helmsman, is the subject of two commentaries analyzed by Thomas Brückner. The twelfth-century school of Chartres saw the *Aeneid* as symbol for stages in the development of humanity and Palinurus as allegory for human susceptibility to error. But error can be recovered and avoided by following correct counsel and the good way. In the *Disputationes Camoldulenses* (1474) of Christoforo Landino, Palinurus, in humanistic thinking, is still the symbol for human error. But Landino provides recovery by human experience and events. Both interpretations are Platonic but with marked differences. The Chartres school saw humanity turning from error to God; the Florentine group saw humanity struggling through error—the active life—toward "dignitus hominis"—the contemplative life.

To determine how death is effected in carnival plays, Thomas Habel analyzes a small but distinctive canon of these works. He concludes that death in the carnival plays is consistent with the general preoccupation of the era with proper readiness for death. Elizabeth Blum, examining death and burial in evangelical hymnody, stipulates that hymns ex-

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pressed categorical, not individual, attitudes, such as passage to eternal life or the imponderable will of God. Death may bring physical relief and even be anticipated as the victory of eternal life. By the turn of the seventeenth century, hymns had changed from the bold proclamations of faith typical of the Reformation to intensely personal inventories of agony. The study of funeral sermons does not readily submit to a quantified methodology, so Paul Richard Blum employs a content-narrative analysis, based on a limited collection at Tübingen. The leading conclusion is that the funeral sermon was not a separate class apart from the main body of sermons. The theme of death is distinguishable only in that the deceased was eulogized and the bereaved were succored. Otherwise, sin, death, and justification by faith were preached in familiar polemical terms.

The investigation by Mary Lindemann into civic death rites accorded undesirables and indigents actually begins with funeral practices of the elite. These rites were conspicuous and sumptuous and remained so through Reformation simplification: the sermon supplanted the ritual. With station and eminence secure in the community, the elite were untouched when required to accord minimal obsequies to the unwanted and improvident. Later, under enlightened reason and sobriety, great houses reduced their unwise expenditures on funeral display. But, whether pompous or pious, the contrast between dying worthily or unworthily is an element of power and control in society.

Power and control also are the motives surrounding three posthumous memorial observances dedicated to Charles V, as interpreted by Achim Aurnhammer and Friedrich Däuble. Details of the celebration in Augsburg reveal Ferdinand's relative weakness vis-à-vis his newness in the imperial office, lack of papal recognition, traditional princely opposition, and suspicion from newly legitimized Protestant states. Philip II in Brussels, despite the Netherlands constitution and noble resistance, had a glorious demonstration-procession celebrating Habsburg grandeur and quite overshadowing the requiem rituals. In Bologna the obsequies were mounted by the Collegio di Spania. Free of political statements, they were not therefore more religious. Their neoclassicism was nearly secular—the orator was the humanist Francesco Robertello—and looked toward the rational funerary inventions of the Baroque.

In sum, the volume offers deeply researched presentations using traditional sources and established methodologies to consider a new subject in Germanic studies. The diversity of the essays shows the complexity of the theme of death in sixteenth-century Germany and the rich opportunities for fruitful research. They also show how far such study is from identifying commonalities, making generalizations, or developing unique methodologies. A concluding unity offered is that the phenomenon of death in sixteenth-century Germany was, variously, a theological phenomenon, a social event, and an institutional tool. Death was almost never a solitary struggle with a personal demon.

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MARILYN J. HARRAN. *Luther on Conversion: The Early Years*. Ithaca: Cornell University Press. 1983. Pp. 218. $24.95.

When did Martin Luther arrive at his new understanding of justification, referred to on the basis of later recollections as the "tower experience"? Although most scholars over the past century have dated this early, before or during his first biblical lectures, the 1513–15 Psalms *Dictata*, some have placed it as late as *Ninety-Five Theses* or even in the ensuing months. Noting that still in the *Dictata* and the following lectures on Romans (1516) Luther used medieval terminology, such as *facere quod in se est*, and stressed a need for repentance and humility by the individual, Ernst Bizer in his *Fides ex auditu* (1958) strengthened the case for the later date. Bizer acknowledged expression of *sola fides* in the lectures but saw in the call for humility still a place for human effort and therefore an element of medieval synergistic views of salvation.

Marilyn J. Harran proposes to shed new light on this scholarly debate by examining the concept of conversion in Luther's early thought. Admitting that Luther used the term itself infrequently, she explores instead three stages in the process of conversion in his thought: preparation for conversion (can one prepare himself or herself?), the nature of conversion itself, and perseverance in conversion (can one, for example, lose salvation?). Harran claims that tracing Luther's steps away from belief in a synergistic conversion to the belief that justification and therefore salvation is God's work, mediated through his Word and received in faith and thanksgiving, results in a clearer understanding of Luther's transition from fearful monk to confident reformer (p. 21). For this story she closely examines his lectures to 1519—Hebrews and Galatians followed Psalms and Romans—and also some of his sermons, letters, and treatises of the period; she concludes with an analysis of his recollections of the tower experience.

Harran's discussion focuses on the first and third stages in the process of conversion, for she recognizes that Luther from the beginning regarded conversion to be Christ's action. She asserts that Luther's thought evolved from confusing and con-

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tradictory statements in the early lectures, which sometimes demanded humility as a precondition of grace and implied perfection as a goal of the Christian life even after justification, to a much more consistent affirmation in the lectures on Hebrews and Galatians that our righteousness is solely accountable to God's mercy. Thus, she sustains Bizer, although she identifies more of the mature Luther in his early thought. She believes her work has validated and made the best sense of Luther's depiction of 1545 of the tower experience, in which he seemed to suggest a period of evolution followed by a late breakthrough around the time of the controversy over indulgences.

This book is an important contribution to our understanding of the beginnings of Luther's reformation. No book, however, is likely to settle the question of how and when Luther came to his mature theology. Besides the words "justified by faith," the ideas of predestination and of a dynamic, even existential, anthropology of the Christian who, *simul justus et peccator*, continues to need God's grace and mercy were already present as early as the *Dictata*, and in my opinion Harran has not considered their consequences seriously enough. In the light of these concepts Luther's students could have understood those statements stressing humility and perseverance—her evidence of a lingering synergism—in a less literal fashion than she allows. Furthermore, some recent scholarship—see David C. Steinmetz's excellent (and more readable) *Luther and Staupitz* (1980)—argues that, when Luther used "humility" and other medieval terms, he used them in a way already bereft of personal virtue. Thus, Steinmetz and company have reinvigorated the argument for an early Reformation breakthrough, and to me this view is more persuasive than Harran's.

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WALTER G. RÖDEL. *Mainz und seine Bevölkerung im 17. und 18. Jahrhundert: Demographische Entwicklung, Lebensverhältnisse und soziale Strukturen in einer geistlichen Residenzstadt.* (Geschichtliche Landeskunde, number 28.) Wiesbaden: Franz Steiner. 1985. Pp. xv, 410. DM 86.

Our profession generally prizes methodological innovation and conceptual originality. Sometimes, however, historians believe that a close adherence to previously established rules of investigation may be more appropriate—as, for example, when they undertake comparative treatments of some era or problem.

A case in point is the study of historical demography in Germany. Modern methods for the analysis of premodern populations were refined in France and England in the 1960s and introduced to Germany soon afterward, above all by Arthur E. Imhof. Under his impetus and influence demographic techniques were disseminated and important projects were launched. Imhof himself has by now largely moved on to other interests and topics, but the methods he introduced and the projects he helped initiate are still thriving in the Federal Republic.

Among these projects is a massive investigation of population trends in Mainz and its hinterland during the early modern era. To date the most important product of this undertaking is the present volume by Walter G. Rödel, which covers the demographic history of Mainz itself in the seventeenth and eighteenth centuries. The author has specifically eschewed an innovative approach to the subject and favors instead what he calls "an exact adherence to the internationally recognized and utilized French methods" of historical demography (p. 278). Despite some significant gaps in the parish registers, especially for the seventeenth century, the author and his team found a wealth of material to work on. In addition to establishing the overall population trends for Mainz, Rödel used family reconstitution methods to determine the fertility history of 280 individual women. The records of a single parish were also probed for information on geographical mobility and the selection of marriage partners. All the results are now presented in a detailed text, bulging with tables, graphs, and appendixes of the kind made famous by the great theses of the *Annales* school.

Essentially, these findings show that until the end of the eighteenth century Mainz had a typically premodern demographic system, with the high mortality and fertility rates characteristic of the *ancien régime*. But the author shows that the average age at first marriage for women was somewhat lower in Mainz than in comparable communities and the average number of children per marriage was somewhat higher. Rödel concludes from this and other evidence that Mainz, unlike some other communities, experienced no family planning before the end of the eighteenth century. He explains this as a result of the inhabitants' rigid adherence to Catholic teachings, which forbade any form of contraception. Mainz was, after all, the seat of the most powerful archbishop in Germany, and clerical influence there was particularly high.

Like any good demographic historian, Rödel draws attention not only to norms but also to extremes: the child bride of twelve, for example, or the woman who bore eighteen children. And his unparalleled mastery of local detail emboldens him

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to question the theories of more speculative historians. The sudden increase in illegitimate births in Mainz after 1780, he explains, need not be interpreted as the result of any revolution in sexual attitudes—it was simply because of the establishment of a hospice to which unmarried pregnant girls from the countryside were sent to have their babies.

Details such as these, however, rarely enliven Rödel's text. For the most part the author simply concentrates on the fundamental data, explains his methods with care, and offers his conclusions with caution. This book is not pathbreaking, but it was not meant to be. It is instead a conscientious monograph that uses the established methods of historical demography to confirm, amplify, and, in some cases, to modify the existing assumptions and axioms of an important field of historical investigation.

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RUTH-ELLEN B. JOERES and MARY JO MAYNES, editors. *German Women in the Eighteenth and Nineteenth Centuries: A Social and Literary History.* (Proceedings of an Interdisciplinary Conference at the University of Minnesota, 1983.) Bloomington: Indiana University Press. 1986. Pp. xviii, 356. $29.95.

This volume, in conjunction with John Fout's recently published *German Women in the Nineteenth Century: A Social History* (1984), provides an introduction to the latest research on eighteenth- and nineteenth-century German women's history. Although seven authors contribute to both collections—in all cases on different aspects of the same topic—articles are not duplicated. Ruth-Ellen B. Joeres's and Mary Jo Maynes's volume originated in a conference that brought together American, British, and German historians and scholars of literature. The editors emphasize the interdisciplinary nature of these collected articles; they place this work in the tradition of women's studies by suggesting the "ways in which the constraints of a single disciplinary tradition have perhaps distorted or limited our perceptions of German women in the past" (p. xi).

The essays are divided among three sections: work and ideas about work for women; feminisms; and women's words and self-definition. The third is the shortest and least cohesive, although the individual articles provide fascinating glimpses into the lives of German women. All three sections address, obliquely, some of the central questions of women's history: What has been the relationship between work and family? How has work affected the lives and consciousness of women? What are the origins of feminist consciousness? What is the relationship between gender and class consciousness? All of the articles contribute in one way or another to the discussion of these issues.

Some make a contribution primarily by introducing unknown or little-known sources. For example, Rosemary Orthmann makes use of an investigation of 1902 into the style and cost of living of unmarried female factory workers that provides information on age, occupation, income, housing, and expenditures for 939 Berlin women, and John Fout describes the testimony of 181 Viennese working women presented to a commission investigating the working conditions in industry in 1896. Other authors make use of published autobiographies, newly discovered letters, travel literature, and similar kinds of literary sources.

Some of the articles offer complex and provocative analyses of large issues through case studies. Jean Quataert, for example, challenges the notion of a universal sexual division of labor by documenting the existence of family teamwork and the sharing of domestic tasks among rural weavers in the Oberlausitz, a Saxon district. Kay Goodman, in an article that belongs as much in the section on feminisms as in the one on work, explores the tension between arguments of "sameness" and "difference" in the women's movement through her focus on Ellen Key's concept of "the misuse of women's energy." In her study of the Protestant deaconessate at Kaiserswerth, Catherine Prelinger shows the complexities of the relationship among the patriarchal family, religion and philanthropy, women's consciousness, and the women's movement. In accord with Hans Adler, who explores a feminist controversy between German Catholic Louise Otto and freethinker Louise Aston, she insists on the importance of religion to the German bourgeois women's movement.

Much more is to be found in this collection. Joeres and Maynes have brought together a relatively focused group of essays based on a variety of sources and written from both literary and historical perspectives. The introduction is excellent, if too brief. The editors suggest that the juxtaposition of the essays enriches them, but they could do more to draw out the central themes that engage the individual authors. The section on feminisms ends with Richard Evans's piece on the use of the concept of feminism by historians. It is a thought-provoking conceptual essay that provides an apt conclusion for the middle section, something that the other sections lack. Yet Evans does not specifically refer to the use of the concept of feminism by the other contributors; the editors might have gone one step further and attempted their own conclusions about the meaning of feminism based on the essays presented here.

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Joeres and Maynes clearly recognize the general issues that the articles gathered here address, but they are perhaps too modest in seeing their work only as a beginning, a chance "to open channels" (p. xv). Certainly the essays do point the way to sources and topics worthy of further exploration. But they do more than that by providing a great deal of information on and analysis of women's work, feminism, and consciousness. Perhaps a preliminary synthesis of existing research on German women's history can now be written.

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RÜDIGER VOM BRUCH, editor. *"Weder Kommunismus noch Kapitalismus": Bürgerliche Sozialreform in Deutschland vom Vormärz bis zur Ära Adenauer.* (Bücher zur Sozialgeschichte und sozialen Bewegung.) Munich: C. H. Beck. 1985. Pp. 283. DM 48.

Rüdiger vom Bruch and his coworkers have provided a useful historical survey of middle-class social reform (*bürgerliche Sozialreform*) from its beginnings in the days of the uprising of the Silesian weavers to the crisis of *Sozialpolitik* during the West German economic downturn of the 1970s. The essays cohere as a book in that, together, they illuminate the contribution of middle-class social reform in Germany to easing the passage to a buffered industrial society and thence to the eventual development of the welfare state. In the introduction vom Bruch proposes four qualities that, taken together, distinguish the history of the German movement: the unique role that middle-class reform organizations played in Germany; the collaboration of these secular and confessional bodies with the state while maintaining no formal or even functional connections with it; the search for a third way, neither communist nor capitalist, which would better divide the benefits of society and resist the importation of Manchester liberalism; and finally, in the course of the twentieth century, the transformation of middle-class social reform into *Sozialpolitik*, which was conducted by the state and influenced by parties and pressure groups.

Jürgen Reulecke's essay on the beginnings of organized social reform in Germany emphasizes the role played by ethically motivated civil servants and industrialists—and the Silesian troubles of 1844—in the launching of the Centralverein für das Wohl der arbeitenden Klassen. Many of its leaders appeared at the Frankfurt Assembly in 1848. The distrust by the Prussian authorities permitted the Centralverein to accomplish little, and its most important members, including Gustav Schmoller, joined the Verein für Sozialpolitik at its founding in 1872.

The essay by vom Bruch on the *Verein*, its more active sister, Die Gesellschaft für soziale Reform (founded 1902), and the Protestant and Catholic welfare organizations covers the imperial period effectively. Concern to find uniqueness in German history, however, leads him into drawing too sharp a contrast between German and British social reform. The late nineteenth-century social reform tradition derived from J. S. Mill bears comparison to the work of the Verein für Sozialpolitik.

Günther Schulz treats the acceleration of the transformation from middle-class social reform to the beginnings of a state social policy (*gesamtgesellschaftliche Sozialpolitik*) in the Weimar Republic. His analysis of the 1923 crisis of *Sozialpolitik* lacks analytical precision. It bogs down in organizational narrative and does not place the crisis in the context of the socioeconomic history of the republic. The transition to state social policy largely completed, the middle-class social reformers lost touch with events and found themselves eclipsed by the parties, pressure groups, and bureaucracies that had invaded their realm. In 1936 Werner Sombart persuaded the members of the Verein für Sozialpolitik to disband the organization because it was no longer necessary.

Michael Prinz, writing on the National Socialist era, describes the dissolution or Gleichschaltung of the remaining middle-class and confessional reform groups. The welfare functions that had passed into the hands of the state under the republic were left in place, and added benefits were conferred by the National Socialists' complete and dramatic solution to the crisis of *Sozialpolitik* through the economic revival stimulated by military expenditures.

Hans Günther Hockerts's contribution completes the story of middle-class social reform with what the authors deem its final success, the achievement of its goal of social pacification in the prosperous social market economy of the Federal Republic. Here, as a subsection of Hockert's essay has it, German society bade "farewell to the proletariat in the welfare state" (p. 262). In the 1970s heightened social costs disturbed the social consensus, but Hockert refrains from judging the long-term trends in future *Sozialpolitik*.

The ensemble of studies builds on the work of Ludwig Preller, Albin Gladen, Walter Bogs, and Volker Hentschel. The essays' contributions lie in the conceptual realm. The interpretation favors the reading of German history of the Bielefeld school. Before 1918 the German bourgeoisie had to attempt to reform German industrial society largely outside the mechanism of the state. Only with Weimar, partially, and then completely in the Federal Republic, could the bourgeoisie employ the state as the instrument of social reform, as in other developed parliamentary welfare states. The vol-

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ume opens another front in the battles over the German *Sonderweg*.

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BARBARA FRANZOI. *At the Very Least She Pays the Rent: Women and German Industrialization, 1871–1914*. (Contributions in Women's Studies, number 57.) Westport, Conn.: Greenwood. 1985. Pp. xii, 206. $29.95.

In the course of the last several decades the leading interpretation of European industrialization has been altered dramatically. In the first place, research on "protoindustry" or rural manufacture in the early modern era has shown that large areas of "agrarian" Europe actually bustled with the making of cloth, tinplates, nails, scythes, and clogs. Second, recent studies also demonstrate that the introduction of new technology, for example, steam power, was neither as sudden, nor as complete, nor as revolutionary as was once believed. Indeed, industrial development is a much more protracted and complex phenomenon than that revealed by the once-dominant linear model of economic growth and concentration. It originated in the first stirrings of dispersed manufacture in the European countryside in the sixteenth and seventeenth centuries, and decentralized, labor-intensive production continued to have a prominent role in economic life well into the era of factory industry. Many historians now are impressed by continuities during the transition to industrial capitalism and stress how workers themselves adapted their culture and rhythms of work to the new environment.

Barbara Franzoi's detailed study of women's industrial work in imperial Germany before the First World War is a fitting complement to this new body of literature. As she describes it, "The dominant theme of this study is the adaptive strengths and strategies women generated as they engaged in a system in economic transition" (p. 185). But it is more. It is a new look at the processes of German industrialization from the perspective of women's continuous need to work for wages as well as for the family (both as daughters and wives). The perspective of gender leads her to describe expansion, particularly of consumer goods industries, through nonmechanized, nonfactory labor; transitions from centralized to decentralized production; and the extent to which factory production required low-skilled, cheap, and flexible homeworkers. She offers a picture of German industrialization that differs greatly from the standard interpretation, and she joins a growing number of historians who stress continuity in the lives of working women as they consistently sought to mold their employment around their domestic world.

Franzoi begins with a useful critique of imperial German occupational statistics, once used by contemporaries to decry the "dramatic" disruptions in women's lives, and then proceeds to look at women's work experiences in diverse industries in different regions of Germany, often using the words of the working women themselves. In this fashion, women appear as agents in the historical tale, who maneuver to turn liabilities into assets. The author traces the interaction of work and family activities over the course of women's life cycles and probes both the motivations for as well as the contributions of women's work in the lower-class family economy. Separate chapters also consider homeworkers and unionization, including a suggestive discussion of the considerable appeal of religious unions to women in the imperial period, clearly a fruitful area for further research, as Franzoi herself notes. The style is dense and, typically, several major points are bunched together in one paragraph, which distracts from the impact of the rich and vibrant details. Also, the causative lines are more complex than Franzoi describes. Domestic production survived—or even thrived—precisely because it found in women a cheap source of pliant labor. Furthermore, the characteristics of women's work long had been in the making, since the emerging capitalist system required divisions of labor by sex and corresponding distinctions between market and household production. To claim that "women's work cannot be merged with men's work analytically" is patently wrong (p. 7). Women's and men's work were distinct in the real world, as Franzoi shows, and only analytically can one understand their complex interaction in the developing capitalist economy.

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ALFRED G. MEYER. *The Feminism and Socialism of Lily Braun*. Bloomington: Indiana University Press. 1985. Pp. xvi, 235. $27.50.

Lily Braun (1865–1916) is, or ought to be, best known as the author of *Memoiren einer Sozialistin* (1908–11), which describe the author's development from a young aristocratic lady, daughter of a Prussian general, into an ardent feminist and finally an active member of the Social Democratic party. The book's seductive power derives not least from its portrayal of the author as a woman who experienced life to the full and so was never really able to subordinate herself to the discipline required by large political organizations such as the feminist or socialist movements, which she suggests were led by

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stiff, narrow-minded, rather limited people who sacrificed much of their humanity to political ends. Braun's memoirs pose a serious challenge to her biographers, of whom Alfred G. Meyer is the first to make full use of her voluminous private papers, housed in the Leo Baeck Institute in New York. How can biographers escape from the spell of Braun's memoirs and provide an account that not only is more accurate but also stands as a viable alternative on its own?

Meyer has chosen to solve this problem by concentrating on an analysis of Braun's ideas, which provides a useful foil to the concentration of her memoirs on her life. In the central chapters of his book he advances a strong case for Braun as a significant precursor of late twentieth-century feminism in her emphasis on sexual and domestic liberation as well as equal rights. He even presents her as a Western neo-Marxist *avant la lettre* because she argued for a feminized Marxism and developed a theory of woman's alienation from femininity, an alienation that would end only with the overthrow of capitalism. Meyer's exposition of these ideas is well worth pondering, even if his attempt to assimilate them under the concept of alienation is rather strained and makes them sound more sophisticated than they really were. Braun's demand that women experience femininity to the full included the elevation of motherhood to the center of the female experience. Between 1914 and her death in 1916 it also led to her praise of war as sweeping away the last obstacles to men's full expression of their virile instincts and women's full realization of their drive to self-sacrifice. By now a militarist and annexationist, Braun ended her career longing for a new leader who would sweep away the dross of the past and place men and women in full control of their respective separate spheres.

To Meyer's credit he brings out these continuities in Braun's thought, but they seriously weaken his claim for Braun to be regarded as a precursor of present-day feminism. Ultimately, the claim suggests perhaps that Meyer has after all fallen under Braun's spell. He says too little about the other feminists of the day whose ideas were similar to Braun's before 1914. And his brief and sketchy version of her life, as opposed to her thought, altogether fails to offer an alternative view to that of the memoirs. Indeed, they have influenced him to provide an account of the Social Democratic party and its women's organization that is a grotesque travesty and to portray Braun's rival Clara Zetkin in terms more negative and one-sided even than Braun did. Following Braun, he sees the feminist and especially the social democratic movements as life-suppressing forces unable to cope with Braun's life-affirming character. This assumption leads to errors too numerous to list here. To take just one example: Meyer comments that Braun's "delight in celebrations and festivals went against the puritan spirit of orthodox Marxism" (p. 107); yet what of the socialist celebrations such as Mayday or the Social Democrats' attempts to organize their women's meetings along the lines of popular festivals, as described by Luise Zietz? Meyer can only accept Braun's own evaluation of such matters because he has not troubled to consult recent research on German labor history. This is a pity, because ultimately Braun's life was a good deal more interesting than her ideas, and a thorough exploration of it set fully in its contemporary social and political context would be useful. Meyer has given us a useful introduction to Braun's thought, but an account of her life remains to be written.

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RONALD WIEDENHOEFT. *Berlin's Housing Revolution: German Reform in the 1920s.* (Architecture and Urban Design, number 16.) Ann Arbor, Mich.: UMI Research. 1985. Pp. xx, 204.

In the past decade the history of German housing has received a great deal of attention from scholars in West Germany and elsewhere. Ronald Wiedenhoeft's book does not lead its readers to this new literature but does add significantly to it. Wiedenhoeft's training in art history makes him an excellent guide to Berlin's new housing of the 1920s. The book is richly illustrated with photographs, site plans, and floor plans of twenty *Siedlungen* (settlements) built in Berlin between 1914 and 1931. Illustrations comprise 40 percent of the text. One addition would be welcome: a map of greater Berlin that locates the *Siedlungen*.

Wiedenhoeft's thesis is that subsidized housing built in Berlin during the Weimar years constituted a revolution inspired and patronized principally by socialists. The aim of the revolution was to build affordable housing for "the poor" (p. 173) and especially for "the low-income working population" (p. 50). Wiedenhoeft describes utopian plans published after the revolution of 1918 by radical architects, and he follows their efforts to design revolutionary but practical housing for Berlin's workers. Until 1928 most of the "progressive" architects favored single-family housing with attached garden plots; thereafter, architects abandoned this model and concentrated on denser projects with smaller apartments and without individual gardens. But, instead of retreating to the closed peripheral designs of the prewar *Mietskasernen* (rental barracks), they built "superblocks," parallel rows of two-to-four-story buildings with ample ventilation and light and with innovative space-saving features. Wieden-

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hoeft clearly illustrates the connections between patronage and design; the most innovative *Siedlungen* were sponsored by socialist-controlled building societies and municipal agencies, whereas more traditional designs prevailed with more conservative sponsors, such as employers and white-collar unions.

It is not clear what sort of revolution was achieved. If the goal was to build for "the poor," then it failed, since most of the new housing was occupied by civil servants, white-collar workers, or steadily employed skilled workers. Nor is it clear how typical Berlin's housing problems were for German cities or how innovative Berlin's new housing was by national or international standards. One can hardly fault Wiedenhoeft that his book will reinforce a longstanding bias toward Berlin in the literature on German housing, but readers should know that housing conditions in Berlin before 1914 were decidedly worse than in most other German cities and that the innovations of the 1920s were consistent with national and international trends. Wiedenhoeft could offer instructive comparisons to the new housing of Frankfurt, the Ruhr, Amsterdam, and Paris, for example, and one hopes that he might find an occasion to do so.

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ROBERT LEWIS KOEHL. *The Black Corps: The Structure and Power Struggles of the Nazi SS*. Madison: University of Wisconsin Press. 1983. Pp. xxxi, 437.

To all students of the National Socialist era of German history, and to every specialist interested in the history of the SS, Robert Lewis Koehl's scholarly contributions stand as familiar works or valuable resources. With the publication of this study, which completes an effort he began more than two decades ago, the author has added an important, long-overdue dimension to serious scholarship on the single most influential institution in the Third Reich. In the range of considerations it includes, as well as through the features and issues it omits, this is a book no serious student of the period should neglect. Like any good volume, it raises as many questions as it attempts to answer.

Six of the eight chapters detail the origins and evolution of the SS from the turbulent aftermath of World War I to the methodical preparations for the destruction of Poland that began the second global conflict. These densely packed pages bear all the anticipated hallmarks of Koehl's scholarship: an unsurpassed knowledge of the major SS personalities and power struggles that shaped Heinrich Himmler's crack militarized police and terror formations; extraordinary insights into the political dealing, personal backstabbing, and bloodletting that fashioned SS relations with the other agencies of the Nazi party and with ministries of the Third Reich; and sound exposition of the methods employed by Himmler and Reinhard Heydrich to gain control of the German police and enlarge the presence of the SS into virtually every area of national life by 1939. The massive documentation, set forth in a running narrative of explanatory footnotes, has almost as much value for the specialist as the text itself. The bibliography is impeccable.

A long single chapter, "Years of Tragic Fulfillment, 1939–1945," carries the wartime narrative down to a final chapter outlining the author's conclusions. This relative imbalance, given the reciprocal importance between the German war effort and the SS, is but one of several difficulties with which the volume leaves the student.

In his introduction Koehl writes: "I hope this is a useful substantial book" (p. x). That, the volume unquestionably is. The real difficulty—though perhaps an unfair criticism—is that many of us expected much more, a work of scope and precision that would prove enduring, rather than a book merely useful and substantial.

The subject of the last chapter, to take but one example, would require immense elaboration; many chapters would be required to fill in a complete picture of the structure and power struggles of the SS. Much of the narrative, and many of the conclusions, look and sound like material and judgments developed long ago and never reconsidered, reformulated, or redigested in light of the historiography of the last decade. The same is true for the lack of any systematic discussion of arguably the most important wartime SS enterprise—the destruction of the European Jews. Auschwitz is barely mentioned, Treblinka and Maidanek (save for a chart) not at all.

Finally, stylistic difficulties and editorial oversight combine to blunt and dull, rather than hone and sharpen, important points in the book. What, to illustrate, is the reader to make of the following conclusion: "In our century fascism seems to be symptomatic of a badly blocked society trying to shake itself loose of the blockages by massive reverberations. Fascists are part of the reverberation mechanism, cast up by the force of the social jerkings and tremors, but their own purpose is to use the reverberations to gain control over the society. Once in command, they seek to control the thrusts and vortexes in the rapidly changing system, yet in so doing they free some of the blockages while choking off adaptive mechanisms in the subsystems of their society" (p. 228).

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BOYD L. DASTRUP. *Crusade in Nuremberg: Military Occupation, 1945–1949.* (Contributions in Military Studies, number 47.) Westport, Conn.: Greenwood. 1985. Pp. xi, 159. $27.50.

Given its thesis, style, and sources, this study of American occupation forces in post-1945 Nuremberg presents the reader with what can only be described as a written form of the "Big Picture," the U.S. Army's long-running television documentary. Although Boyd L. Dastrup cites some recent interpretations and accounts of the occupation, his book leaves the following impression: Rising from a mysterious forty-year sleep, Nuremberg detachment officer Colonel Rip Van Winkle decides to bare all to the American public. It is his duty to inform us that the occupation in Nuremberg was a mixed success.

In highly compartmentalized chapters the author describes the Americans' efforts to implement such policies as denazification, reeducation, media control, reconstitution of political parties, and economic revival. Seldom do the various policies interact or interfere with each other. Friction among the various offices and echelons of military government is rare. Dastrup stresses the theme that the Americans had to use authoritarian means to achieve democratic ends. Denazification, he notes, was imperfect and unpopular; reeducation through educational reform disrupted the school system and foundered because of public resistance; the control of the military government over the German press limited freedom of information and free speech; and the Nurembergers resented the dismantling of factories for reparations and the industrial inefficiencies caused by the breakup of large cartels. Overall American policy, the reader learns, was initially punitive. Then, by 1947, it began to change in light of the cold war and humanitarian considerations, and by 1948 it was avowedly reconstructionist. Needless to say, these revelations are scarcely new.

Although this study earnestly tries to give a comprehensive picture of American occupation policy in Nuremberg as influenced by brutal reality, its effect is curiously flat. Seldom does the reader see events beyond the borders of the once-proud capital of Franconia. Thus, when Dastrup describes German resistance to proposed school reforms, the reader comes away with the impression that the Nurembergers simply would not stand for changes in so basic a social institution. Nowhere does the real architect of this resistance, the reactionary cultural minister for Bavaria, Alois Hundhammer, appear in the book. Surveys of public opinion conducted at the time and interviews today with eyewitnesses from the occupation would have done much to alleviate the heavy official bias of this work. In Dastrup's book the Nurembergers largely remain silent.

Dastrup faithfully describes the vacillating American occupation policy, and his local history approach is meritorious, although he should have better integrated Nuremberg's plight into that of postwar Germany. I wish that Dastrup had done the job with greater skill—the book suffers from embarrassing typographic errors and misspellings throughout. This book is actually useful for students of the occupation as a reference on the implementation of policy in the occupation. Its sharp limitations, however, make it painful going for any but the most determined reader.

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MARITA KRAUSS. *Nachkriegskultur in München: Münchner städtische Kulturpolitik 1945–1954.* Foreword by GEORG KRONAWITTER. Munich: R. Oldenbourg. 1985. Pp. 402. DM 68.

The book's subtitle is more accurate than the title, since Marita Krauss deals with the cultural policy of the city, for which ample materials are at hand, not with the concept of culture or the relative merits of Munich's postwar cultural offerings. Krauss tries to present "an approximate overview of the bewilderingly many-sided spectrum of efforts and initiatives of cultural policy" (p. 227)—an apt description. After 1945 Munich's citizens wanted to revive traditional "bourgeois" culture as a source of legitimate, nonpolitical values.

The book contains six parts: a biographical sketch of Hans Ludwig Held; a general analysis of cultural policy; and chapters on city policy toward music, theater, literature (libraries, publishing, writing), and pictorial arts (galleries, museums, exhibits). In addition to his position as city librarian, Held was head of the Department for Culture as special commissioner for culture and was charged with the "promotion of Munich as a city of art and culture" (p. 31). Although he lacked good contacts with or much feeling for the younger generation of writers and artists, Held was nevertheless the key figure in setting cultural policy, and his papers constitute Krauss's most important source. Most of the city's financial support went to major cultural institutions such as orchestras, theaters, libraries, and museums, since Munich found it difficult to aid individual creative artists and writers, and the book's primary focus is in fact the allocation of city funds to those institutions.

Krauss is mildly critical of Munich's reputation as a city of the arts. She feels that opportunities were missed, that cultural efforts were too conservative, too institutional, too bureaucratic. Munich's failure to do anything to attract the publishers from the

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Soviet Zone was clearly a missed opportunity. Munich was a much-desired spot, but the city housing office was uncooperative, and neither Held nor the mayor's office was able to provide housing and facilities to aid and encourage relocation there. The lack of housing also discouraged any influx of new writers and artists.

Krauss's criticisms, however, are often buried under the mass of detail. Indeed, for a study of the postwar years, this account is remarkably passionless, even lifeless. Only an occasional aside, such as a mention of the cancellation in April 1948 of the programs of the city theaters because of mass influenza (a result of poor housing and food for the actors), reveals the extraordinary character of the times. Krauss argues that party politics played little role in cultural policy, even in such actions as the purging of the public libraries of books tainted by Nazism (25 percent of the collections) or in restrictions on certain authors because of their writings under the Nazis, and that city policy marched in perfect harmony with the American occupiers, who seldom intervened in cultural matters. This picture is too simple. Why were the mayor and Held unable to persuade the housing office to support their cultural policies? In fact, throughout Germany the reconstruction, repair, and allocation of housing brought out bitter political strife. Krauss sees as "a typical example" of American policy "the melting down of the printing plates for *Mein Kampf* for the first edition of the *Süddeutsche Zeitung*" (p. 49). But American policy was rather more ambitious than that, as for example in the attempts to reform the school system. Finally, because of the limited perspective in this book, many potentially interesting issues, such as conflict between generations of artists and writers, are not explored. Much more remains to be done on culture in postwar Germany.

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CHRISTIAN PFISTER. *Das Klima der Schweiz von 1525–1860 und seine Bedeutung in der Geschichte von Bevölkerung und Landwirtschaft.* Volume 1, *Klimageschichte der Schweiz 1525–1860*; volume 2, *Bevölkerung, Klima und Agrarmodernisierung 1525–1860.* (Academica Helvetica, number 6.) Bern: Paul Haupt. 1984. Pp. 184; 163. Tables 34.3; 7.2.

A great deal has been written during the last decades about European weather anomalies prior to systematic weather observations (1659 in England, 1864 in Switzerland). This research points to better growing seasons during the High Middle Ages and substantially cooler conditions beginning with the late sixteenth century. The work has been of highly variable quality and tended to overgeneralize from sparse or local data. Christian Pfister has now provided a model study, based in good part on exhaustive archival research, which yielded some thirty-three thousand weather records (comprising eighty thousand observations), and has critically processed them on a monthly basis, differentiated for thirteen areas within Switzerland. These records include specific weather observations and measurements as well as proxy data such as dates for the blooming and maturing of certain plants, official dates for grain auctions and grape harvests, information on quality and quantity of the wine yield, and observations of glacier fluctuations, location and duration of snow covers, high- and low-water stages, and other indirect consequences of weather.

The Swiss data are spotty for the period 1525–49, increasingly diversified during the seventeenth century, and abundant beginning in the mid-eighteenth century. In his first volume Pfister meticulously sorts out these materials by computer and presents estimates of deviations in temperature and precipitation (against twentieth-century averages) for each month and year since 1525. Longer-term trends that extend to all Switzerland emerge from the curves of eleven-year means and include: a warm climate (1530–64); a cooling trend (1565–1629), intensified after 1601 and with unusual variability from 1585 to 1616; dry winters and springs (1630–87); extremely cold winters after 1677, with maximum cold, primarily winter and spring, from 1688 to 1701 (climax of the "Little Ice Age"); warming springs and longer, drier summers (1702–30); cold winters and warm, increasingly wet summers (1731–1811); and a second, very cold period (1812–60), with cool summers and early frosts. Pfister then draws on other European evidence to reconstruct broader weather patterns for several anomalous years and to provide generalizations more valid than have previously been possible for temperate Europe.

In his second volume Pfister tackles the more difficult problem of interpreting the impact of these climatic trends and shorter perturbations with respect to agricultural and demographic trends. He has a special advantage in that the timing, duration, and quality of each growing season can be estimated with considerable precision. He begins by investigating the systemic feedback relationships between nutrition and mortality; he then evaluates the potential productivity of the different components of the past agricultural systems (grains, grapes and other fruits, and milking), their contributions to diet and monetary economy, and strategies for minimizing risks in the various Swiss economic sectors. Consequently, in reconstructing the changing productivity of cereals, wine, and dairy products over time, he is able to avoid the pitfalls of simplistic correlations. Furthermore, Pfister focuses on the

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weather of the growing season, changes in agricultural technology and strategies, longer-term demographic trends, as well as the interdependence of all these factors.

Subsistence crises, associated with sharply reduced births and increased mortality, were found to be associated less with longer trends than with short runs of extremely cold springs and wet summers (in 1569–74, 1586–89, 1593–97, 1626–29, 1688–94, 1769–71, 1816–17, and 1854–55). Trends in per capita agricultural output were indeed linked with more persistent climatic anomalies but cannot always be isolated from positive changes in agricultural technology. Pfister has also identified qualified relationships between agricultural output and trends of population growth or decline, although the demographic data are spotty. For example, population growth was stimulated by good growing seasons in the periods 1530–59, 1632–87, and 1720–68, although the pace of growth was utlimately dominated by agricultural modernization during the mid-eighteenth century and, especially, the nineteenth century. The overall result is a cautious interpretation of climatic impacts on agricultural productivity and demographic change, presented in the context of a first-rate analysis of agricultural history.

This major work deserves widespread attention by interdisciplinary historians. The extended summaries in French and English, in conjunction with the graphic data and models and the copious numerical documentation in the appendixes, should not preclude its use by anglophone readers.

KARL W. BUTZER
University of Texas

ANDREAS SUTER. *"Troublen" im Fürstbistum Basel (1726–1740): Eine Fallstudie zum bäuerlichen Widerstand im 18. Jahrhundert.* (Veröffentlichungen des Max-Planck-Instituts für Geschichte, number 79.) Göttingen: Vandenhoeck und Ruprecht. 1985. Pp. 436. DM 90.

During the 1720s the rural communities along the northern reaches of the Jura Mountains successfully rebelled against the episcopal absolutism of the ecclesiastical princes of Basel, developed autonomous, democratic forms of government, engaged in an independent foreign policy, and, for a brief time, developed an alternative social vision to both the absolutism of their prince and the traditional forms of peasant life. Andreas Suter's "case study" is a broad investigation into the events and peasant actions of a rebellion that occurred amid changes in imperial and French politics and in the regional and European economies, whose inexorable progress together undermined and destroyed this attempt at an independent peasant republic. The study seeks to dispel an image of peasants as merely reactive and irrational, as reactionary and violent, seduced into opposition by an outside leadership. It is true that the peasants' movement was a reaction to the usurpations of the episcopal state, they did follow an urban magisterial leadership that invoked legal documents nearly two centuries old, and they engaged in many violent acts. Nonetheless, Suter recasts this story in a comprehensive and generally satisfying explanatory framework that does justice to the rational, independent, progressive, and irenic dimensions of the peasants' activities. This is clearly a significant addition to the history of eighteenth-century rural politics in an area where the economic, military, political, and social practices propping up the *grosse Politik* of the empire, France, and the Swiss cantons may be observed.

Extractive industries, smelting, and large-scale livestock farming yielded the highest rates of return in the region's economy, and the episcopal princes began after about 1650 to appropriate the necessary land and forest resources to control these sectors. In the process they built an alternative body of property law that contradicted and undermined communal use rights in favor of the state's claims of eminent domain. By the early eighteenth century the peasants found that their economic base of mixed farming, which depended on the use of the commons, could no longer compete in the most profitable sectors. The authorities' overgrazing of the commons, the emergence of an advantaged class of householders who served as the state's agents in the villages, the termination of common access to the woods, and the forced settlement by the authorities of immigrants who did not acquire full communal status and were not bound by communal rules compelled the peasant leadership to take extreme action in defense of their right to use communal property: "To the degree that the bishops in the course of their political interests had to get rid of the village commune, so the peasants in turn, recognizing their interests, had to get rid of the state" (p. 337).

While their leaders were attempting to create a politically unified regional movement, most of the peasants pursued their goals in their immediate communities through tax resistance, violations of game laws, expulsions of outsiders from commons, reclamations of land and other resources, charivaris, fence and roof dismantling, "house running," and so on. This, the heart of Suter's work, is based mostly on administrative documents from the episcopal archives at Pruntrut; the many microanalyses he contributes alone make this book essential reading. He provides useful tables and lists. Serious problems, however, mar Suter's approach and arguments; he underplays aspects of his sources that

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do not fit his argument; the social analysis is weak, which leads to weak appriasals of the significant roles women and youths played; and he does not explore the relationship between the French-German rivalry in the villages and rebellion. He also advances a Malthusian argument that undercuts his best sociopolitical and economic analyses.

Suter's greatest problem is to understand the mental processes behind both the state's and the peasants' actions. He demonstrates, for example, that the state's authoritarian impulses undermined the democratic-associative practices of the regional estates and communes and produced social and ideological divisions that split the commune movement and rendered it authoritarian and oppressive. Nevertheless, because he ascribes a moral economic, "safety first" mentality to the Basel peasants, he undercuts the force of his argument concerning the "rationality" of the peasant movement. The rationality he ascribes to the peasants diminishes them, finally, in the face of what he insists was the "necessary" systematic rationality of the bishops. By his own evidence this argument is clearly insufficient; the peasants' willingness to negotiate for a statewide tax reform on the basis of progressive taxes, for example, cannot be fully explained by a moral economy argument. Ultimately, one question remains: if the historical actions we have assumed, until recently, to be irrational and criminal turn out to have been matters of rational choice, then where has the irrational gone?

HERMANN REBEL
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CARLO GINZBURG. *The Enigma of Piero: Piero della Francesca; The Baptism, the Arezzo Cycle, the Flagellation*. Foreword by PETER BURKE. Translated by MARTIN RYLE and KATE SOPER. London: Verso; distributed by Schocken, New York. 1985. Pp. 164. $16.95.

Carlo Ginzburg's engagement with the art of Piero della Francesca at first appears to be a surprising interest for this historian renowned for his brilliant examinations of popular culture. Yet Ginzburg's less widely known considerations of art historical methodology, especially concerning the relevance of political history to art, provide a background to this book, which first appeared as *Indagini su Piero* (1981).

However welcome is Ginzburg's plea for "forays," such as his present book, into the field of art by the general historian, and however hopeful one is that Ginzburg will provide some working alternative to the farrago of details and speculations found in the Piero literature, the author does not entirely fulfill one's expectations. Perhaps this is due to the high standard by which we judge Ginzburg's work. And we must be mindful that the study of Piero's paintings, by necessity grounded in a meager fund of documented fact, is one of the most unsettled and elusive subjects in Renaissance art history. One is nevertheless disappointed that Ginzburg's book is founded, no less than is the work of art historians, on relentlessly pursued, intricate chains of evidence, much of it speculative and thus of varying credibility.

A special difficulty is that Ginzburg has in effect written two books: one intended to elucidate several works by Piero, another meant to demonstrate in broad terms the relative certainty of a history of art based on the examination of patronage. A consequence of the attention to patronage is Ginzburg's extensive reference to, and review of, previous interpretations, thereby obscuring his own valuable hypotheses concerning Piero.

Drawing largely on published materials (by Creighton Gilbert, Maria Tanner, and Thalia Gouma-Peterson in particular), Ginzburg argues that the *Baptism of Christ* (London, 1440), the frescoes illustrating the *Legend of the True Cross* (Arezzo, circa 1453–59), and the *Flagellation* (Urbino, 1459)—the dates are Ginzburg's—constitute a distinct phase in Piero's career. A common intellectual source for the paintings is situated in a "network" of humanists whose chief figures were Ambrogio Traversari and Cardinal Bessarion. These people are linked to Piero through Giovanni Bacci, an Aretine humanist and a member of the family that commissioned the *True Cross* murals. Ginzburg believes Giovanni to be responsible also for commissioning the *Flagellation*.

The interest of the patron and other humanists cited by Ginzburg in reconciling the Eastern and Western churches and mounting a crusade against the infidel Turks informs, in varying ways, the paintings under consideration. The Council of Florence (1439), the fall of Constantinople (1453), and the Council of Mantua (1459) provide a framework for dating the works. The study of patronage and iconography thus solves, according to Ginzburg, vexing questions of chronology with more secure results than those attained through stylistic analysis.

A much-abbreviated recapitulation of Ginzburg's exposition does little justice to a book that manifests a command of the most esoteric facets of Piero's world and an astonishing control of a considerable scholarly literature. The connection of specific contemporary events and important Roman humanists with the Arezzo frescoes and the *Flagellation* appears certain. Ginzburg's case nevertheless is in need of additional corroboration for the ingenious assignment of a crucial role to Giovanni Bacci and would also be helped by more extensive reference, if possible, to other works of art that allude to issues raised

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at the aforementioned church councils. The belief that the *Baptism* is an illustration of ecumenical thought and the somewhat self-serving identification of presumed portraits in the *True Cross* murals and the *Flagellation* are the least cogent parts of the study.

Ginzburg's book is the first systematic attempt to trace the links between Piero and his social and political matrix. Yet Ginzburg tends to exaggerate the influence of Piero's setting on the style and themes of his art; the Piero who emerges in the middle decades of the fifteenth century appears virtually to be an invention of Bacci and his associates. The investigation of Piero's patronage, as Ginzburg has shown, is important and fruitful. No discussion of the wellsprings of Piero's art is complete, however, without some recognition of the artist's formidable independent intellect and creative invention.

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GENE BRUCKER. *Giovanni and Lusanna: Love and Marriage in Renaissance Florence.* Berkeley and Los Angeles: University of California Press. 1986. Pp. x, 138. $13.95.

In 1980, while working in the Florentine State Archives, Gene Brucker found, scattered through the twenty volumes of the notary ser Filippo Mazzei, acts relating to a marriage suit, heard before the archiepiscopal court in the summer and autumn of 1455. A woman of comparatively humble origins, Lusanna, accused the patrician Giovanni della Casa of bigamy. Giovanni had recently married Marietta Rucellai, but he allegedly had married Lusanna, then a widow, in May 1485. Lusanna contested that she and Giovanni had exchanged nuptial vows at the home of her brother in the presence of a Franciscan friar and twelve witnesses. The couple, however, drew up no dotal agreement, allegedly because Giovanni feared the disapproval of his patrician relatives. Later, the witnesses had difficulty remembering what day in May the wedding occurred, and two of them denied that it had ever taken place in their presence.

In the course of the trial Giovanni and his witnesses did their best to denigrate Lusanna's character. Even before her husband's death she allegedly took lovers. Giovanni admitted that he was one of her lovers but not that he had ever married her. In spite of Giovanni's denials, the court, on November 29, 1455, decided in Lusanna's favor and ordered Giovanni to recognize her as his legal wife. Giovanni immediately appealed to the pope and received some sort of reply. Unfortunately, the reply was lost and its contents are unknown.

In this short book Brucker retells this tale of love and deception in Renaissance Florence. He also offers numerous, always brief, comments on love, marriage, women, class relations, and politics under the rule of the Medici. The text, unencumbered by footnotes, gives no exact references to the acts scattered through the notary's twenty volumes and no translations of their contents. Should historians in the future want to rehear this trial, they will have to start from scratch. The book does offer sketches, inspired by contemporary drawings but redesigned to serve the purposes of the narrative; they are chiefly sexual in theme. Besides the illustrations, the chief attractions of this book are its readable style and the inherent interest of the tale it relates. Brucker's book proves beyond a doubt that narrative history is back. When may we expect the movie?

DAVID HERLIHY
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H. C. BUTTERS. *Governors and Government in Early Sixteenth-Century Florence, 1502–1519.* New York: Clarendon Press of Oxford University Press. 1985. Pp. xviii, 350. $39.95.

With wit and good humor, and a solid grasp of detail, H. C. Butters defends old-fashioned narrative history as an effective literary strategy for explicating the intricacies and illuminating the dark corners of Florentine politics. Patronage, clientage, and a host of political maneuvers and tactics designed to raise revenue and support public credit were no mere epiphenomena. In fact, they were the essence of civic life, not what Butters calls "smoke above the factory." Clearly, he perceives little of value in the overdetermined sociology of an Emile Durkheim or the structuralism of Fernand Braudel. Butters's narrative account of political life in Florence begins with the election of Piero Soderini (who nicely observed legal limits) to gonfalonier of justice for life in 1502, through to the return of the Medici in 1512, and on to the ill-fated domination by Lorenzo di Piero di Lorenzo Medici (d. 1519). The need to manage the patriciate of the city, to impose taxes, and to hold public support for a costly foreign policy became increasingly burdensome. Because it went far beyond the unassertive management techniques of Cosimo and Lorenzo the Magnificent of the past century, the rule of the Medici from 1512 to 1519 provoked bitter opposition.

To investigate changes Butters pursued a trail of documents in the Florentine archives and libraries, as well as those in Mantua, Modena, the Vatican, and Rome. His rendering of the documents is sure, inspiring confidence in the reader; further, he uncovers significant pieces of new evidence. What does not surface, however, are new insights or new hy-

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potheses to be tested. The political landscape charted in this narrative is flat, and the characters portrayed, one-dimensional, with little exploration of possible links between private and public behavior. Like others in the field of Florentine political and constitutional history, he makes civic life virtually synonymous with the activities of the patriciate. We see their passion for legality often being negated by the urge to evade the rigors of the constitution in matters pertaining to voting, political appointments, eligibility for office, and sharing power with the lesser folk of the city. The Florentines appear to have been unable to live with or without their laws and constitution. The actors in Butters's political drama have little sustained interest in cultural or religious life and are not securely rooted in commerce and banking. Even the economic position of Florence and its vital trade with France and other parts of North Europe do not receive full measure. Geography and economic dependency did not allow Florentines the wide berth of negotiation open to other city-states—especially Venice. Republican traditions, as well as parsimony, worked to disadvantage Florence militarily, and the city was without a proper standing army. The author's efforts at comparison are not always enlightening; for example, those pertaining to the Florentine nobility and that of North Europe make little of the differences between bastard feudalism in England and the clientage and patronage of Florence. A world of indentures is certainly markedly different from a world of casual favors and civic benefactions. Was the Florentine experience of the domestication of the magnates a commonplace? That the Florentine government did so little to advantage the nobility is surely a mark of difference between Venice and Florence. Does not the mapping of comparative social arrangements allow for the charting of different landmarks? Does not the small scale of patrician retinues and of Florentine courtly culture merit comment?

The problem with this monograph does not stem from the author's radical empiricism that casts doubt on efforts at social analysis and political comparison. Instead, the difficulties emerge because he fails to use the narrative voice to dramatize that what at first sight may seem irrelevant, accidental, and contingent is in fact the very stuff of real political life. No such humanized version is presented as a serious challenge to the overdetermined view of politics the author rejects. Despite his repudiation of analytical and comparative methods, he has not advantaged himself of the benefits of the narrative. Storytelling is no privileged and innocent art.

MARVIN B. BECKER
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BARRY COLLETT. *Italian Benedictine Scholars and the Reformation: The Congregation of Santa Giustina of Padua.* (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University Press. 1985. Pp. x, 287. $46.00.

This learned and careful study of the Benedictine Congregation of Santa Giustina has a relevance that its narrow and seemingly recondite subject does not immediately suggest. Recent scholarship has shown, for instance, that, contrary to what we used to believe and despite some immense and unresolved problems, Italy was experiencing in many respects a powerful religious revival during the fifteenth and sixteenth centuries, a revival sometimes related to the humanist movement. The revival was, in any case, quite different from the corresponding phenomena in Northern Europe and different to a large extent from what would follow in Italy after the Council of Trent. The burden of Barry Collett's study is to describe and document one aspect of this earlier Italian reality.

The book is developed along the following lines. After the appointment of Ludovico Barbo as abbot of the Paduan monastery of Santa Giustina in 1408, that institution underwent a thorough reform, soon grew to attract some two hundred talented and devout members, and joined to itself a number of other Benedictine houses (including, eventually, Monte Cassino itself) to form a large "congregation." Characteristic of this revival was its gentle piety and especially its devotion to patristic scholarship, which Collett terms humanistic. Collett's special concern is to trace and delineate the teaching of the congregation on salvation (or "justification" or soteriology). In so doing, he contrasts it with the teaching on the same issue of the Protestant Reformers, the Italian *spirituali*, and the Council of Trent. He concludes by showing how and why the teaching of the congregation failed to prevail and eventually was dissipated.

The central and most specific thesis of the book is that the teaching that the monks proposed was derived directly from their reading of pre-Augustinian, Greek fathers of the church. This theology was different from the current "ascent" theology, with its Pelagian dangers. It was, rather, a "theology of the Cross" but lacked the forensic framework typical of Western posings of that position. Thus, it provided a formulation that could have helped the church out of its apparently hopeless impasse over the relationship between grace and works. In the course of this discussion, Collett throws considerable light on "the nature of Italian Evangelism" and on its principal work, the much-studied *Beneficio di Cristo*. Benedetto da Montova, the author of the original version of the *Beneficio*, was, after all, a respected member of the congregation.

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Collett describes his book as "intellectual history" (pp. 6, 10). He overcomes the limitations often found in the genre by his good contextualization, and he demonstrates, *ex opere operato*, that knowing what leading figures think about the issues that agitate their age continues to be important for all historians. Granted, the central thesis is technical, and key ideas recur in the book with regularity. In summary, however, this is an original and welcome study. We have in it documentation concerning a solid and widespread monastic revival in Italy before the Reformation, concerning the breadth and depth of the patristic revival there, concerning Italian evangelism, concerning the rich and varied soteriologies at play in the cinquecento, concerning those mysterious "Benedictine abbots" present at Trent, and concerning a process whereby a strong intellectual tradition suffers demise. Finally, when one finishes the book, one almost perforce has come to share Collett's open admiration "for such scholarly, humane, and courageous men" (p. viii).

JOHN W. O'MALLEY
Weston School of Theology

BARBARA MCCLUNG HALLMAN. *Italian Cardinals, Reform, and the Church as Property.* (Publications of the UCLA Center for Medieval and Renaissance Studies, number 22.) Berkeley and Los Angeles: University of California Press. 1985. Pp. xii, 232. $35.00.

Barbara McClung Hallman examines fiscal records to trace the acquisition of benefices, ecclesiastical pensions, and church offices by 102 Italian cardinals and their retinues during the period of church reform in the sixteenth century. Not surprisingly, she finds pluralism, nepotism, and widespread use of church incomes and offices to supply patronage networks controlled by these cardinals and their relatives. The church provided careers, incomes, pensions, and opportunity for social advancement to many of Italy's most prominent families, and she argues *à la* Denys Hay that the Italians' loyalty and protectiveness toward their family interests undermined reform of church property holding.

She concludes that the various attempts to reform abuses in the distribution and management of ecclesiastical incomes, most notably the *Consilium de emendanda ecclesia* of 1537, had a marginal impact and that most cardinals, including members of the reform commission itself, continued to regard church property as a source of personal income rather than a sacred trust to be used solely for pious ends. The author contends that early concerns in this area were swept over by the tide of events that redirected the energies of the church toward the more pressing issues of theology, doctrinal purity, piety, and the founding of new orders.

To isolate general trends in the patterns of acquisition and alienation of church property up to the end of the Council of Trent, Hallman divides the sample of cardinals into three chronological groups according to date of promotion. Nonresidency and the holding of plural episcopacies, highly visible abuses and long a focus of reform, were substantially reduced by mid-century, but the less-visible conflation of lesser benefices intensified. In addition, other practices, such as garnering pensions, reserving incomes from resignations, and renting out benefices, increased toward the end of the century. The growing monetization of ecclesiastical incomes, as evidenced by the datary's expansion and the proliferation of church annuities through colleges of offices and *monti*, opened new opportunities for investment and speculation to anyone who had money.

These kinds of fiscal practices were not unique to the church and could be found in various stages of development in most premodern states. They were directly related to burgeoning bureaucracies and rulers' attempts to pay for the rising costs of government and of war with new forms of credit and deficit financing. Oddly, however, in the name of reform, historians are quick to reprimand the church for just such common practices. Hallman is no exception. Because she approaches the distribution of church property from the standpoint of measuring actual practice against the stringent ideals of reformation, anything that concerns money becomes tainted with abuse. She acknowledges that cardinals were expected to maintain large retinues and often incurred heavy costs in the performance of their duties. They had to pay their expenses with incomes from their benefices. But she makes no distinction, as surely the sixteenth-century reformers did, between reasonable use and flagrant abuse. Undoubtedly both existed among the group of cardinals she studied.

MELISSA MERIAM BULLARD
University of North Carolina

ORAZIO CANCILA. *Baroni e popolo nella Sicilia del grano.* Palermo: Palumbo. 1983. Pp. 238. L. 25,000.

Orazio Cancila, who has gained a reputation as Sicily's leading economic historian of the Spanish period, has written a valuable and well-documented study of the island's history between 1450 and 1600. The author shows that the expansion of wheat production at the expense of stock raising and of other crops in the sixteenth century distorted economic development and benefited only the feudal landowners and their associates. Firmly rooted in

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Sicilian archival sources, the work is a critical synthesis of recent scholarship in the field and offers the reader a general picture of the economy supported by an impressive array of quantitative data.

In the first half of the book Cancila examines the long-term trends: the slow expansion of the late fifteenth century, the rapid growth of the early sixteenth century, and the onset of agrarian crisis in the 1570s. The first chapters reveal the very different world of late medieval Sicily that was characterized by thickly wooded coastal regions, opportunities for foreign immigrants at all social levels, and the confinement of cultivation largely to the coastal areas. Cancila usefully illuminates the secular trend with data on population, production, consumption, and exports of wheat from the island. In analyzing the late sixteenth-century crisis, he provides evidence of the temporary fall in the area of sown land, of cultivators' strikes, of declining numbers of bovine livestock, and of increased mortality in many parts of Sicily. As he observes in chapter 6 (in which he discusses tools and crop yields), reliance on oxen rather than horses crippled Sicilian agriculture. The dry Sicilian climate made the cost of feeding horses prohibitive, and Cancila blames adverse climatic conditions rather than excessive beef consumption for the sharp decline in the number of oxen in the late sixteenth century. Although the main emphasis is on trends relating to wheat, other products—sugar, vines, olives, and silk—also receive extensive treatment.

In the later part of the work Cancila discusses the effect of the swing to wheat monoculture on the island's "barons" and "people." The chief beneficiaries were clearly both the established baronial families and newcomers to their ranks who bought feudal estates, and associated with them were the middlemen, often nonnoble—foreign merchants, for example—who managed their estates. Despite the favorable economic conjuncture, however, many noble families were beset by financial difficulties, a situation facilitated by the *soggiogazione*, or mortgage contract. In Cancila's opinion such families were incapacitated above all by their need to consume conspicuously at whatever cost. The mass of Sicilians, on the other hand, were the victims of the age: the independent smallholder largely disappeared and the massive influx of the dispossessed into Palermo caused grave urban problems. The relatively brief, though suggestive, treatment of their fate (pp. 194–215) reflects the limited amount of research that has been done so far. Notwithstanding the lack of any maps and an abundance of obscure place-names, the book is indispensable for all students of the Mediterranean in this period.

TIMOTHY DAVIES
London

MARTIN CLARK. *Modern Italy, 1871–1982*. (Longman History of Italy, number 7.) New York: Longman. 1984. Pp. 444.

Martin Clark seeks to provide a new synthesis of Italian history, an endeavor in which he mostly succeeds. Focusing on the eternal conflicts of post-Risorgimento life—state versus society, "legal" versus "real" Italy, high versus low politics—Clark indicates that "the main themes of Italian politics have remained remarkably constant since Unification" (p. 2). The central difference between past and present, in his view, is that the costs of absorbing and transforming the various contending forces have escalated, given the shift from an elite-based polity to one founded on universal suffrage and on democratic assumptions (or, better, pretensions).

Although Clark's method is essentially chronological, he accomplishes his main purpose through a series of set piece essays (mostly organized as subchapters), which are often executed very skillfully and sometimes verge on brilliance. In compiling a list of particularly insightful discussions, I found myself itemizing thirty or more such headings. These include: a powerful analysis of postunification Italy—Italietta—that incorporates a nearly anthropological viewpoint and provides a picture of late nineteenth-century Italy as a classical case of a backward society (and in some respects an almost primitive one); a balanced and insightful discussion of Italian industrialization that focuses on the complementary roles played by state intervention and private initiative and puts to rest some of the standard arguments about the tariff policy of 1887; a striking essay on Fascism as essentially a revelation of previous Italian history and especially as an outgrowth of the pre–World War I crisis and of the war itself. In addition, Clark provides a fine discussion of what he calls the "dullard" interpretation of Fascism (and to which he rather self-deprecatingly subscribes): the view that the triumph of Fascism in Italy was not inevitable and that Mussolini's regime cannot easily be compared with other European dictatorships. "Italy was very much *sui generis*," in Clark's words. "The only way to understand why Italy became Fascist is to study its detailed history" (p. 260).

One could extend the list of particularly valuable subchapters and self-enclosed essays in this volume to include the ones on Mussolini and on the question of consensus ("there was acceptance Fascism, but not devotion, consensus, but not commitment" [p. 247]), on the tenuous and short-lived accomplishments of the Resistance, on the rise of Christian Democratic hegemony and the subsequent secularization of Italian life, on the contradictions of the Constitution of 1946 and the central role of the civil service after World War II, on the continuing theme

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of regionalism and the north-south differential, and on shifts in the Italian family and in the role of women. Suffice it to say, Clark's history is broad-based and includes much more than just politics (indeed, Clark notes that social and economic issues have always become fodder for political debate and accommodation). Clark's attempt at inclusiveness, however, proves both a defect and a virtue. Given the proliferation of essays, the narrative and chronological thread sometimes becomes attenuated and one often has difficulty in tracing the underlying story from year to year and over decades. This, in fact, might not be deemed a substantial drawback by the specialist, and Clark's insights—and his mordant humor about things Italian—are often a delight for the well-versed reader. His method, however, remains confusing for the beginning student (as my own undergraduates attest). Equally confusing are Clark's use of English equivalents for standard Italian titles (Humbert I for King Umberto, the Association of Christian Workers for ACLI, and so on), and his occasional use of untranslated Italian phrases.

It is ultimately not clear what sort of book Clark intended to write: the present stimulating (but somewhat episodic and disjointed) account or a textbook useful as an introduction to the course of Italian history over the past century. If it be the first, he has succeeded rather admirably. If the second, we must reluctantly still have recourse to the standard (and by now rather dated) texts.

DONALD HOWARD BELL
Tufts University

OREST SUBTELNY. *Domination of Eastern Europe: Native Nobilities and Foreign Absolutism, 1500–1715*. Montreal: McGill-Queen's University Press or Alan Sutton, Gloucester, England. 1986. Pp. xii, 270. $30.00.

This cleverly constructed, vigorously argued, and generally well-written book focuses on early modern Eastern Europe, defined at the outset as "the area between Russia and the Germanies" (p. x), which turns out to mean Poland-Lithuania, Livonia, Hungary, Moldavia, and Left-Bank (eastern) Ukraine. Bohemia, the other Baltic territories, the lands of the Southern Slavs, and the rest of the Balkans are thus excluded apparently to accommodate the book's central thesis. Orest Subtelny asserts that in the late seventeenth and the early eighteenth centuries the areas in question witnessed the culminating crisis in a general confrontation between two "inherently contradictory political systems": the "foreign" absolutist states of the Habsburgs, Ottomans, Romanovs, Vasas, and Wettins, on the one hand, and the native "noble associations" of this "Eastern Europe," on the other. Subtelny further suggests, mostly by allusion and implication, that this clash was of epochal significance for the peoples (not just the nobilities) of the territories involved—indeed, for all of Europe.

To Subtelny the absolutist powers appear invariably aggressive and predatory, whereas the nobilities were the hapless defenders of liberty (their own). Yet he also makes it clear that the nobilities were on the whole recalcitrant, xenophobic, exploitative minorities—tiny minorities—within their respective societies. More basically, they were the product of an "economic detour": Eastern Europe's regression in early modern times into "agrarianism" (the standard or historical norm assumed throughout, in political, social, and economic developments, is an undifferentiated "Western Europe" or "the West"). Subtelny does, however, credit the absolutists for at least claiming to act for the common good—"the most effective device in their struggle against the nobles" (p. 107). And in exile the various leaders of the failed nobiliar rebellions "eventually acknowledged that strong monarchical rule was a more effective form of government than the associative systems which they had sought to defend." This is "a tribute," Subtelny says, to their "intelligence, dynamism, and commitment" (p. 208).

This is not a work of original research: the nearly three hundred fifty notes to the text contain scarcely a dozen references to primary sources (none are mentioned in the preface or bibliography). Nor is it, obviously, a textbook, meaning a systematic history of early modern Eastern Europe. Nor does it entirely succeed as a synthetic or interpretive essay—the fault of some fairly naive theorizing, forced argumentation, factual errors or inconsistencies, and substantive contradictions (for example, "in 1721 Russian rulers began the systematic liquidation of Ukrainian autonomy" [p. 137], but "Peter I [died January 1725] systematically liquidated Ukrainian autonomy" [p. 164]). Yet, as an essay in comparative history, built on five case studies, I found the book consistently interesting; the details drawn from a selection of the monographic literature are fresh and often suggestive in their juxtaposition, and the linking generalizations are often stimulating. I say this even while concluding that Subtelny has not advanced an understanding of the Ukrainian aspect of his subject beyond that achieved in his previous publications.

JAMES CRACRAFT
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PASCHALIS M. KITROMILIDES. *Iōsēpos Moisiodax: Oi syntetagmenes tēs Balkanikés skepsēs ton 18O aiōna* [Iosepos Moisiodax: The Coordinates of Balkan

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Thought in the Eighteenth Century]. Athens: Cultural Foundation of the National Bank of Greece. 1985. Pp. 396.

First under the Byzantine empire and then under the domination of the Ottoman Turks, Southeastern Europe had long been separated from developments in Western and Central Europe. During the eighteenth century, however, intellectual currents associated with the Enlightenment worked their way eastward so that the people of the Balkans experienced, largely under Greek direction, their own version of the Enlightenment. Iosepos Moisiodax, the subject of Paschalis M. Kitromilides's study, played an influential role in this movement.

Born in Dobrudja (circa 1725) and probably of non-Greek origin, Moisiodax nonetheless identified closely with Greek culture in his personal life and scholarly activities. Little or nothing is known about several extended periods of Moisiodax's life, and his career has generally been studied by philologists. Kitromilides's social-scientific approach is therefore an important contribution to the existing literature.

Moisiodax, as many of his learned contemporaries, apparently pursued a peripatetic path in his educational development. He studied and interacted with scholars and teachers in places such as Janina, Thessaloniki, Smyrna, the Athonian Academy, Vienna, and the University of Padua. Along the way he became a deacon in the Greek Orthodox Church. He spent a good part of his career teaching and writing in Jassy and Bucharest, which had excellent reputations as centers of Greek education under the Phanariote Greek court.

Moisiodax concentrated on transmitting a number of the main ideas of the Enlightenment to the Balkans. He was greatly influenced by John Locke's ideas on education and argued that European education, with its increasingly secular and rational content, provided the appropriate prototype for the cultural transformation of Hellenism. He stressed the new findings in modern mathematics and experimental physics, advocated the empirical approach, rejected Aristotelianism, and promoted "sound philosophy." Thus associated with the "moderns," Moisiodax inevitably encountered the opposition of the conservative "ancients." This conflict was particularly acute on the issue of language. Whereas Moisiodax used modern Greek in his teaching and writing, most of his colleagues continued to emphasize classical Greek and the use of texts for grammatical exercises.

Although Moisiodax's adherence to the theory of enlightened despotism is generally accepted, Kitromilides offers evidence that he embraced more advanced ideas for the period. Moisiodax was an admirer of England, but he appreciated the development of political institutions in nonmonarchical areas of Europe, such as Switzerland. The thought of Jean Jacques Rousseau evidently made an impact on Moisiodax. He, in turn, passed it on to students, among whom was Rigas Velestinos, an early heromartyr of the Greek revolutionary cause against the Ottoman empire. With his ideas on education and society, Moisiodax served as a catalyst for change in the Balkan, and not exclusively Greek, world during this nascent period of nationalism.

Kitromilides is representative of a growing number of European- and American-trained academics who have returned to Greece and are now producing first-rate scholarship on modern Greek history, society, and politics. Whereas such studies infrequently merited translation in the past, the quality of many of the newer studies is such that scholars and students of the Balkans and modern Greece in the United States and Europe can only benefit from translated editions.

S. VICTOR PAPACOSMA
Kent State University

D. KOSÁRY *et al.*, editors. *Études historiques hongroises 1985.* In three volumes. (XVI[e] Congrès International des Sciences Historiques, par le Comité National des Historiens Hongrois.) Budapest: Akadémiai Kiadó. 1985. Pp. xvi, 728; vi, 695; xiii, 216. $79.00.

Ever since 1931 the Hungarian historical community has been publishing various collections of essays and bibliographical compendia to celebrate the periodic international congresses of the historical sciences. As the dean of Hungarian historiography, Domokos Kosáry, explains in the foreword to this publication, these collections have been greatly expanded during the last several years, beginning with the Rome congress in 1955. The most recent of the collections is also the most substantial, with over sixteen hundred pages containing sixty-five essays by as many contributors. The set also includes an entire volume listing Hungarian historical books and articles published beween 1974 and 1984.

The sheer magnitude of this effort tempts one to continue playing with statistics. Of the sixty-five essays thirty-three are in German, twenty-four in English, seven in French, and, surprisingly, only one in Russian. Of the sixty-five authors, ten are listed as academicians; sixteen are from the Institute of History of the Hungarian Academy of Sciences; twenty are from the University of Budapest; five are from the Institute of Party History; and the rest are from various other Hungarian universities and academic institutions. The topics treated include historiography, archival science, the teaching of history through film and television, numismatics, archaeology, ancient and medieval history, church history,

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the history of the labor movement, the history of the Hungarian emigration, and the history of other countries (for example, French foreign policy in 1792 and the security and military policy of medieval Russian princes). Only two articles deal with Hungarian developments after World War II.

Was all this worth the effort and lavish presentation? Hardly, I am afraid, because the publication is extraordinarily rigid and unimaginative. According to the text on the dust jacket, the aim of the editors was to "bear witness to the endeavor of Hungarian historiography to re-think—and quite often 're-evaluate'—certain historical problems." This has been partly accomplished by Kosáry in his fine foreword, written in French; however, it amounts to only three pages. This goal may also have been achieved by some of the contributors, but their individual messages are hopelessly lost in the mountain of materials presented. Undoubtedly, present-day Hungarian historians have attempted to rethink and reevaluate a number of momentous issues: national identity, the revolution of 1848–49, the Age of Dualism between 1867 and 1918, the achievements and shortcomings of the Horthy era, Hungary's controversial role in World War II, the history of Hungarian Jews and anti-Semitism, liberalism, fascism, and left-wing radicalism, the problem of national minorities in Hungary and of the Hungarian minorities in the successor states, the democratic experiment after 1945 and the Stalinist take over, the revolution of 1956, and others. Many of these issues are presented in the individual articles assembled here; still, one looks in vain for even the trace of a debate or the citation of divergent opinions. The many greats of Hungarian historiography who wrote these essays—the younger generation is conspicuous by its absence—seem to have been content simply to present their papers. No attempt has been made to connect the essays with one another, much less to present a cohesive whole.

Quite incredibly, the sixty-five essays were published not in chronological order or according to subject matter but, instead, in alphabetical order of the contributors' names. Thus, an article entitled "The Role of Rivers and Mountains in the Medieval Practice of Creating Place-Names in Hungary" directly follows an essay entitled "East Central European Cities and Modernization in the 19th Century," and "The Legal Status of the Bulgarian Colonies in Transylvania (1690–1848)" is paired with "The June 1953 Political Turn and Hungary's Intelligentsia." Because no system, starting point, nor even an index have been provided, one can do no more than draw the reader's attention to such remarkable individual contributions as András Gergely's essay on the liberalization of Hungarian political life between 1830 and 1848; Péter Hanák's masterful study of the attempted bourgeoisification of the Hungarian nobility in the nineteenth century (a development that proved to be a tragic failure and left Hungarian society with a troublesome legacy); György Litván's article on left-wing Hungarian political emigration in Vienna after World War I; Zsuzsa L. Nagy's contribution on the liberal opposition in Budapest during the interwar years; Ferenc Szakály's stimulating essay on Turkish rule and the Reformation in sixteenth-century Hungary; and István Vida's open-minded discussion of the rise and fall—or rather, forceful suppression—of direct, grassroots democracy in Hungary between 1944 and 1948. There are other fine contributions as well, but the overall picture remains fuzzy.

In view of the above, it is a relief to turn to volume 3, which contains a detailed bibliography of recent Hungarian historical publications. These, at least, are systematically presented. Unfortunately, the Hungarian titles have been translated only into French and Russian, and titles that were originally published in a Western language have only been translated into Russian. In view of the essentially Western orientation of Hungarian historiography (witness the presence of only one article in Russian), the insistence on the Russian translation of the bibliographical entries and the lack of translation into English or German are puzzling.

Rumor has it that no such monumental publications are being planned in Hungary for future international historical congresses. This is an excellent decision, for concentration on smaller, more revealing publications would be wiser. Organized topically, these future *Études Historiques* may draw an accurate picture of the work of Hungarian historians, perhaps the most lively and enterprising group of scholars in present-day Eastern Europe.

ISTVAN DEAK
Columbia University

TOMÁŠ VOJTĚCH. *Mladočeši a boj o politickou moc v Čechách* [The Young Czechs and the Struggle for Political Power in Bohemia]. Academia. 1980. Pp. 198. Kčs. 30.

Tomáš Vojtěch surveys in six chronologically organized chapters the development of the Young Czechs from their origins in the early 1860s as a radical faction in the Czech National party, through their organization as an independent party in 1874, to their emergence as the majority Czech party by their victory in the Reichsrat elections of 1891. Although Vojtěch thoroughly examines party organization and propaganda in towns and countryside up to that victory, he does not explore its consequences for local, district, and provincial self-government. His preoccupation with politics in Prague and Vienna may account for his reticence

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about the weakness of Young Czechs in Moravia and how that weakness may help explain their strength in Bohemia. By stopping his narrative 1891, he is not obliged to explain the dissolution of the Young Czech party after 1898 and the subsequent loss of its hegemony in Czech politics. He concludes by unconvincingly asserting that the increasing opportunism and declining advocacy of democracy by the Young Czech party in the 1890s are adequately explained by Lenin's theory of the social regression and growing selfishness of the European bourgeoisie in the period of transition from competitive capitalism to monopoly capitalism and imperialism (pp. 180–01).

Vojtěch's monograph is based on thorough research in extensive Czechoslovak archival collections and on almost all Czech-language publications that treat his or related topics. He builds on the scholarship of Czechoslovak Marxist historians, especially Jurij Křížek and Jaroslav Purš, but curiously omits from his bibliography some equally pertinent works by Jiří Kořalka and Zdeněk Šolle. He summarily dismisses recent English-language studies of the Young Czech party as preoccupied with the development of bourgeois nationalism. Apparently he has not read them carefully because few are so preoccupied, and some anticipated many of Vojtěch's conclusions, including his view that the Young Czechs primarily represented the interests of an educated, propertied elite and his contention that most allies mobilized by the Young Czechs in 1890 and 1891 were politically more radical than the predominately moderate party leadership.

Among aspects of Young Czech history from 1882 to 1891 that Vojtěch discusses more thoroughly than other historians are the Russophilism of leading Young Czechs, the relationship between the Young Czechs and the nascent Agrarian movement in Bohemia, the contributions of various political and nonpolitical affiliates to the growth of the party, factionalism among and conflict between Young and Old Czechs, Young Czech electoral organization and tactics, and the Young Czech part in opposing the *punktace* of 1890. Impressive too are his survey of the rationalization and strengthening of party organization under chairman Emanuel Engel in the early 1890s and his comparison of the Young Czech party with its contemporary Serbian and Austro-German counterparts.

Less complete and satisfactory is the author's discussion of social and economic developments, the character and motivation of party leaders, and Young Czech advocacy of increased civil liberties. He ignores the question of women's emancipation, on which Young Czechs held differing views. He does not clearly define the political interests of the social classes he discusses, perhaps because he anticipated the publication of Otto Urban's *Česká společnost, 1848–1918* (1982). Vojtěch presents industrialization and urbanization as processes that conditioned social and political change but does not consider the influence of private corporations, political parties, or the civil bureaucracy on these processes. Individual Young Czechs are usually mentioned only as members of party factions or as advocates of particular class or occupational interests. Consequently, the book is sometimes lifeless and tedious to read. So detailed are the author's descriptions of factionalism and so little does he speculate on other than material motives that he often portrays Czech political history as no more than a series of ignoble intrigues for selfish interests.

The author is fair-minded in his assessment of Czech and German political parties with two exceptions. On page 132 he dismisses "the movement of so-called national (Czech) workers of the years 1891 to 1893" without explaining what he means by "so-called" and begins a discussion of "Christian Social demagoguery" in which he never indicates that Christian Socialism might be anything more than "demagoguery," even while correctly noting its lesser appeal to Bohemian Czech than to Austrian German voters.

The author provides an adequate bibliography but, regretfully, no index for a detailed book that only specialists in modern Czech history will wish to read cover to cover.

BRUCE M. GARVER
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OWEN V. JOHNSON. *Slovakia, 1918–1938: Education and the Making of a Nation.* (East European Monographs, number 180.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1985. Pp. xvii, 516. $40.00.

Focusing on the state's secondary school system, Owen V. Johnson has written a detailed and thoughtful study of the development of a nationally conscious Slovak intelligentsia and its placement in Czechoslovak society between the two world wars. After introductory chapters on Slovakia before 1918 and the interwar Czechoslovak setting, Johnson concentrates on the network of gymnasia, some of the more specialized secondary schools, and on the composition of their student bodies. Less attention is given to postsecondary education (essentially Komenský University and Slovak students enrolled in institutions of higher learning in the Czech lands) and to such related sociocultural developments as learned and cultural organizations, publishing, libraries, museums, and theaters.

Perhaps only the experienced researcher of Eastern Europe will fully appreciate the vast amount of

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time and effort the author expended to collect his comprehensive base of facts and statistics, which were drawn primarily from the Slovak archives and the secondary schools' annual reports. But the scholarly yield certainly justifies the investment. Carefully sifted and reconciled, the author's data throw new light on Czech-Slovak relations and on the growth of Slovak intellectual life and nationalism between 1918 and 1938. In the process a number of time-worn, poorly substantiated, often politically manipulated myths and stereotypes are seriously undermined. The official "Czechoslovak" state concept of nationality appears to have been far less of a threat to the Slovaks than its critics have made it. The author finds little evidence that Czech "colonialism" (the large contingent of Czech officials and administrators sent by the government to help Slovakia function) was responsible for Slovak unemployment. The Slovaks themselves steadily gained positions in Slovakia as they acquired the necessary qualifications and experience. There is no evidence of a deliberate policy of Czechoslovak governmental discrimination against the new Slovak intelligentsia in vocational and professional placement in the new state. Ultimately, that consciously Slovak intelligentsia—produced (unintentionally) by the strong secondary school system and numbering some thirty thousand persons—became, in itself, a major deterrent to the acquiescence of the Slovaks in the concept of a single "Czechoslovak" nationality.

This volume may also serve as a basic reference tool for researchers working on related topics and questions, some of which the author himself indicates. His wealth of political, economic, social, religious, and cultural data is codified in sixty-four tables, eighteen graphs, and three maps. A bibliographical essay surveys and characterizes the available sources, and a forty-eight page list covers archival holdings, documentary collections, and published literature. The 116 pages of endnotes are, in themselves, an interesting and informative parallel text.

Clearly written, with careful definitions and helpful periodic summaries, painstakingly researched, and consistently careful in its analysis and even-handed in its treatment, this study should be ranked among the most valuable scholarly publications on Slovakia in any language.

JOSEPH FREDERICK ZACEK
State University of New York, Albany

ULRICH KLUGE. *Der österreichische Ständestaat 1934–1938: Entstehung und Scheitern*. Munich: R. Oldenbourg. 1984. Pp. 157.

Ulrich Kluge's probing analysis of the origins and nature of interwar Austria's authoritarian phase provides nourishing food for thought. His interpretive effort focuses on social, economic, and political trends and conditions, rather than events. Throughout his intricate discussion Kluge alternately uses and assesses much of the relevant literature in German (with some attention to a handful of works in English).

Kluge attacks what he sees as customary overemphasis on foreign influence. In his view the dictatorial *Ständestaat* resulted neither from a right-wing conspiracy nor from desperate imitation of neighboring fascist regimes. Emphasizing continuity of domestic problems and perceptions, Kluge sees dictatorship as an organic outgrowth of long-range factors such as enduring sociopolitical conflicts, the lingering trauma of postwar turmoil, the failure to create a generally accepted political system, economic stagnation, and a persistent "corporative" (*Kammer*) mentality. Kluge downplays the events of July 1927 and concentrates instead on political and economic changes during the period of "incubation" after 1929, especially on Engelbert Dollfuss's use of presidential support to erect a reactionary *Expertokratie*.

In addressing the government of Kurt Schuschnigg, Kluge repeats many of his basic arguments about causative factors and the importance of domestic conditions then examines the "possibilities" and "limits" of the authoritarian system. He argues that Schuschnigg's economic and social policies further widened the inherited gap between "society" and "state," especially for the rural and industrial working classes. Observing that the constitution of 1934 was only pseudocorporative, he laments that Schuschnigg, who established a *Kanzlermonokratie* and foolishly opted for the "German Way," failed to foster the development of corporative pluralism, which Kluge thinks might have been possible as late as 1936–37. Schuschnigg's misguided economic policies did as much, however, as his dictatorial predilections and his illusions of an Austrian-centered Reich to undermine support for the *Ständestaat*.

I find the book both stimulating and irritating. Kluge occasionally sets up straw men for the sake of argument, and some of his contentions contain contradictory emphases. For example, he points out both the traditional elements of political behavior and the impact of sociopolitical polarization but then berates the inadequacies of both the party-dominated parliamentary system created in 1920 and, in exaggerated fashion, the *präsidial* system introduced in 1929—overlooking the successful political system Austrians have built on that same constitutional legacy, admittedly under different circumstances, after 1945. Further, he depicts Austria's drift toward authoritarianism as inexor-

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able but then condemns both Dollfuss and Schuschnigg for their personal and political inadequacies. It seems to me, moreover, that in his revisionist ardor Kluge, who, for instance, virtually ignores the foreign patronage of *Heimwehrextremismus*, significantly understates international pressures. Finally, since Kluge's work is in part historiographical, it might be appropriate to note that in 1971 R. John Rath opened for debate some of the same issues in his article "Authoritarian Austria," in *Native Fascism in the Successor States, 1918–1945*, edited by Peter F. Sugar.

Because of stimulants and irritants alike, scholars should certainly want to work through Kluge's challenging arguments. His critique will long inform studies of Austria in the 1930s.

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Davidson College

RICHARD C. LUKAS. *The Forgotten Holocaust: The Poles under German Occupation, 1939–1944*. Lexington: University Press of Kentucky. 1986. Pp. x, 300. $24.00.

The German conquest and occupation of Poland in World War II have spawned a substantial body of scholarly literature that continues to expand at a steady pace. In-depth studies by experts on the history of those dark years have joined the impressive numbers of survivors' memoirs and published documentary collections to produce a mass of printed material that threatens to overwhelm the reader interested in this subject. When the linguistic obstacles are added to the sheer weight of numbers, the task of the researcher appears quite formidable. Indeed, developments have proceeded to the point that the field is in need of a solid academic work that synthesizes the available scholarship in one comprehensive and comprehensible volume, without sacrificing either quality or utility as a "guidebook" to information and interpretations concerning the Polish wartime experience.

Richard C. Lukas appears to have nearly fulfilled this need. Not only has he systematically examined virtually every serious publication in German, Polish, and English on the topic but he has also unearthed some hitherto-unused but important documents from the archives of the wartime Polish government in London. Without omitting any details central to his basic purpose and thesis, he has managed to digest all this material and fashion a book that is relatively short but eminently readable. Briefly stated, Lukas's thesis is that the suffering and devastation endured by the Polish Christian population was as horrible an experience as that endured by the country's Jewish inhabitants—and also every bit as fiendishly and systematically pursued by the Germans. Although in no way minimizing the agony of the Polish Jews, Lukas spells out in convincing detail the social, economic, cultural, and demographic destruction faced by Poles of all social strata, income groups, and geographic locales.

Several salient points emerge from his discussion. One is that the bitter partisan political struggles of the interwar period carried over into the wartime years. The result was almost constant disunity and conflict between competing factions in both the political and military arenas—a condition that was equally prevalent in the government abroad and the underground organizations within the country. This not only severely hampered military operations against the Germans—often rival partisan groups fought each other rather than the conqueror—but also hindered the effectiveness of Polish diplomatic action among the Allies. In addition, regional divisions made it nearly impossible to coordinate military or political activities among the central, southern, and eastern portions of the General Government. Another subtheme is the extraordinary complexity of the military resistance movement. Lukas includes a full discussion of the Communist partisan operations in this section and demonstrates how the suffocating nature of the German occupation effectively limited the underground forces to sabotage and gathering intelligence—at which they were quite good—until the final stage of the conflict. Finally, the author looks at the role of the Roman Catholic Church and its hierarchy during this period and reveals that certain ranking clerics aided the Germans both actively and passively, while the papacy stood by silently.

One major theme of the book is the relationship between Polish Christians and Jews; this subject evokes Lukas's most controversial hypotheses and conclusions. In the two separate chapters devoted specifically to this topic and those portions of others that include discussions of it, the author examines this relationship on four distinct but interrelated levels: individual, communal and societal, political, and military. In the process he demolishes the widely held belief that the Polish political and military establishments practiced widespread discrimination against the Jews and highlights the active Jewish role in the government-in-exile and the underground military organizations. Lukas further notes the overt pro-Soviet position of some Jews, the various criminal acts perpetrated by Jews as well as Christians, and the hostility of leading Jewish groups in America and Britain toward the Polish government as contributing factors to anti-Semitism in Poland during the war. Finally, he chronicles the numerous examples of Polish rescue efforts on behalf of Jews, despite the enormous risks involved and the difficulty in concealing most Jews because they were unassimilated and consequently tended to

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stand apart from their Polish compatriots. Although his observations and conclusions may not be welcome to some readers, they merit serious consideration by those seeking an objective and balanced treatment of this explosive subject.

In sum, Lukas has produced a book destined to have a major impact on future studies of wartime Poland or any other modern society under the stress of military occupation. My only regret is the author's failure to include materials in Russian and Ukrainian published in the Soviet Union, as well as materials in Hebrew, in his sources. Given the complex and emotional nature of his topic, their inclusion might have placed his conclusions on an even more solid footing. Nonetheless, Lukas has succeeded in fashioning a study that should stand the test of time and close scrutiny.

EDWARD D. WYNOT, JR.
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ROMAN BÜHLER *et al. Schweizer im Zarenreich: Zur Geschichte der Auswanderung nach Russland.* (Beiträge zur Geschichte der Russlandschweizer, number 1.) Zurich: Hans Rohr. 1985. Pp. 519. 58 FR.

Much work has been done recently on Russia's relations with other national cultures and the role of foreign elements in Russian development. The old cold war view embodied, for instance, in W. Keller's *East Minus West = Zero* (1962) has increasingly given way to a picture of international interchange, of shared or parallel national experiences within a common context. A particular aspect of this reinterpretation is the question of personnel—foreign individuals and groups; we now know, for example, much more about the German and British communities in imperial Russia. This volume by Roman Bühler *et al.* provides similar information on Russia's Swiss expatriates, but it goes well beyond previous studies in both methodology and coverage. It is the fruit of a research project under Carsten Goehrke on Swiss emigration to (and return from) Russia and South America. The flow to Russia effectively stopped in 1917, when revolution also caused a flood back to Switzerland. The study thus ends with 1918, and data collected by the Russian-Swiss Association created in that year to help repatriates, as well as the repatriates' personal information, have been invaluable to the project. (No access to Soviet archives has been achieved, however.) Computer analysis has been applied effectively. The nearly four thousand *Russlandschweizer* whose personalia are analyzed may be, as the authors say, only a small part of the whole: nevertheless, their data provide a sufficient analytical base. This volume, a broad survey and provisional summary of findings, precedes other more specially focused studies. After a technical introduction, the authors examine the history of Swiss migration to Russia; the migrants' origins and reasons for migration; professions in the new country (the most significant were merchants, confectioners, and cheesemakers); lifestyles in Russia, including much on confessional complexities; and *Wechselbeziehungen*—the importance of the Swiss in Russia and of Russia to Switzerland—as exemplified by representatives ranging from Franz Lefort and the great eighteenth-century mathematician Leonhard Euler to the important nineteenth-century winegrower Louis Vincent Tardent and his Bolshevik great-grandson and to entrepreneurs such as Alexander III's tailor Heinrich Vollenweider and his heirs. The statistical apparatus is amply balanced by vivid case studies; not only individuals but the compact communities and institutions of the Russian Swiss—rural colonies, urban societies and congregations, businesses, schools, and welfare organizations—are brought to life in words and accompanying illustrations. The concluding chapter attempts to place the project's findings within the framework of Swiss emigration at large and of migration theory in general.

Altogether, this is an exemplary study. A team effort using modern techniques to encompass a diffuse and complex historical phenomenon, it is a firmly Swiss book. But it avoids national bias and is grounded in sound procedures and well-balanced perspectives—a welcome change from much other writing of this kind. The book is also admirably judicious, and it claims no final answers at this stage. There is little to fault: some propositions on patterns of migration need more evidence, and the anecdote on page 278 attributed to Jacob Sievers may actually belong to Diderot (see *Mémoires pour Catherine II*, mémoire XXXVIII). Otherwise, we shall simply hope that succeeding volumes maintain the same high standard.

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URS RAUBER. *Schweizer Industrie in Russland: Ein Beitrag zur Geschichte der industriellen Emigration, des Kapitalexportes und des Handels der Schweiz mit dem Zarenreich (1760–1917).* (Beiträge zur Geschichte der Russlandschweizer, number 2.) Zurich: Hans Rohr. 1985. Pp. 460.

Foreign industry and foreign entrepreneurship in Russia have been covered intensively in the literature of Russian economic history. Urs Rauber's study of Swiss foreign investment, Swiss labor, and Swiss entrepreneurship in Russia is a welcome addition to this literature. Rauber's work covers a variety of subjects and touches on a number of disciplines;

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it will serve as the definitive work on Swiss economic activity in tsarist Russia. The author provides case studies of the major Swiss companies operating in Russia, estimates the relative volume of Swiss foreign capital, and studies the life styles and family practices of Swiss entrepreneurs and artisans in Russia. Rauber effectively uses archives in Switzerland as well as the contemporary press (both Swiss and Russian). In sum, this book provides a complete account of Swiss industry in Russia.

Readers from different disciplines will focus on different aspects of this work. As an economist, I was not interested in the sociological accounts of life of Swiss immigrants in tsarist Russia. I was, however, quite interested in the author's finding of a large discrepancy between Russian and Swiss sources for the sum of Swiss investment in Russia. In light of the difficulty of getting reliable estimates of the volume of foreign investment in Russia, a careful case study that uses both Russian and Swiss sources sheds some light on this confusing issue and warns us to be careful with the existing estimates, which rely primarily on Russian sources. Another interesting point is the author's finding of relatively small differentials in wages earned by Swiss craftsmen in Russia over wages in Switzerland. This fact, in itself, may explain the small number of Swiss craftsmen in Russia, but it also creates an economic puzzle as to the motivations behind the immigration. Most other studies find that craftsmen were attracted to Russia by substantial wage differentials, and Swiss wages probably did not differ substantially from those in other European countries.

A carefully crafted and comprehensive study, this book is of interest to specialists in the history of Russian industrialization and in the history of foreign entrepreneurship.

PAUL GREGORY
University of Houston

STEVEN J. ZIPPERSTEIN. *The Jews of Odessa: A Cultural History, 1794–1881*. Stanford: Stanford University Press. 1985. Pp. ix, 212. $32.50.

Steven J. Zipperstein's monograph covers the cultural life of Odessa's Jews from the founding of the city to the great wave of pogroms that struck Russia's Jews in 1881 following the assassination of Alexander II. In 154 pages of text he surveys migration by Jews to the new city, the institutions they created, the pattern of their economic activity, and the content of their cultural and intellectual life as they evolved over nearly a century. This narrative, in turn, is set against the background of the city's geographical setting and historical development.

Zipperstein's description seeks to draw together a number of dynamic factors to explain what he considers a unique page in the Russian and Russian-Jewish experience. The core of the volume rests on his consideration of Odessa as a "discordant, individualistic modern metropolis," (p. 154) guided by commerce and unrestricted by many of the traditional features of older Jewish communities in the Pale of Settlement or by the attitudes of the Russian state toward the Jews. Driven by contacts with the outside world and carrying prospects of social mobility reminiscent of the West, Odessa appeared to its Jewish immigrants to be destined to play the role of model of the future of Russia.

The result of Zipperstein's efforts provides us with a broadened sense of the Russian empire, which differs from the often narrowly viewed perspective of Moscow and St. Petersburg in its Russian component and from Vilna in its Jewish component. Here a picture of increased diversity emerges out of the frontier setting of Odessa. One is struck by new opportunities to draw comparisons with other frontier experiences since newcomers arrived in the city and established themselves in ways that extended beyond the limits of expectation accepted elsewhere in the empire. Perhaps more powerfully than in other parts of the empire, the sense and meaning of modernization permeated the city's growth and its Jewish inhabitants' outlook on life.

The study is executed as a loose amalgam of social and intellectual history. This path is partially dictated by the limited nature of the source materials available. Zipperstein relies heavily on the periodical press in Hebrew and Russian to extract events and attitudes. He switches from social analysis to biography to symbolize positions and achieve a concrete, human perspective. At times this becomes an uneven process, but one that restricted access to materials may have made unavoidable. Nevertheless, Zipperstein's conception enriches our understanding of Russian Jewry and imperial history and provides an example of a useful model for further study of the Russian south and other peripheral areas of Russian history.

HENRY J. TOBIAS
University of Oklahoma

IU. I. SMYKOV. *Krest'iane srednego Povolzh'ia v period kapitalizma: Sotsial'no-ekonomicheskoe issledovanie* [The Peasants of the Middle Volga Region in the Period of Capitalism: A Socioeconomic Investigation]. Moscow: Nauka. 1984. Pp. 230. 3 r.

From the perspective of Soviet historiography, this study of the peasants of Kazan', Simbirsk, and Samara provinces in the years following the emancipation of 1861 shows little originality. Iu. I.

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Smykov adheres strictly to views taken from Lenin, whom he cites frequently. According to Smykov, rural Russia was increasingly influenced by capitalism, despite the persistent effects of a feudal-servile system of agricultural production that inhibited economic development. By the end of the nineteenth century capitalist agrarian structures dominated the countryside. For the peasants of the Central Volga region this meant growing social and economic stratification marked by the impoverishment and proletarianization of a substantial portion of the rural population coupled with the rise of a comparatively small but well-to-do class of village exploiters.

Smykov begins with a succinct statement of Lenin's views and then presents a rather good survey of the archival and published sources. The materials appear to be rich and abundant and could probably be put to much better use. The heart of Smykov's argument is that, although the Central Volga region generally conformed to what he calls the regularity of the historical development for European Russia, two distinct forms of rural capitalism emerged. Southern Samara, less densely settled, had more fertile lands and lower rents on land and brought much grain to market. In fact, rented, sharecropped, and privately owned peasant lands often proved a more important source of revenue than communally allotted lands. Wheat was the major crop, and commercial animal husbandry was important. The peasants were rich in land, and they were primarily responsible for the development of rural capitalism. In contrast, peasants in Kazan', Simbirsk, and northern Samara were forced to undertake various crafts or work as wage laborers to make ends meet. Their standard of living was lower, and theirs was not the impetus behind the rise of rural capitalism, which was inhibited by the vestiges of serfdom. Here progressive former serfowners and merchant-landlords played a greater role in agrarian development, although their success was noticeably less than in the south.

The ideological framework of Smykov and many other Soviet historians who have studied the postemancipation peasantry has inclined them to emphasize the social and economic aspects of village life, especially the emergence of tensions among various strata of peasants. But such historians in their approach are largely analogizing from the industrialized world, and their judgments are often incorrect. In particular, ecological and demographic determinants are reduced to simple variations on the capitalist theme. Moreover, Smykov fails to appreciate the complexities of the use of land in much of Russia and the Central Volga region. Although the peasants were members of repartitional land communes, they were also tenant farmers who ran their farms largely on patriarchal, family lines. They were a long way from capitalist agriculture, and Smykov presents no evidence to support his central point that any stratum of peasants was able to consolidate its power.

Such unfounded statements reflect a serious methodological error common to many Soviet studies of peasant society. Smykov's methods for analyzing stratification are quite simply wrong. Socioeconomic stratification is not the uneven distribution of wealth and status at a single moment in time, as he assumes, but is differentiation over time, giving rise to separate strata with distinct members, each stratum having its own economic interests, social orientation, strategies for survival, and means for maintaining or improving its status. Even worse, for some inexplicable reason Smykov's measures are not consistent (see tables 23–26 and 33). And, to complicate matters further, his analysis of stratification focuses on the household alone and ignores possible changes in its size and structure. Per capita measures would have been more useful. Thus, with all his data, Smykov has shown only that larger households had more livestock, rented more land, and possibly hired more labor. These households did not constitute a wealthy, exploitative rural elite. Per capita measures would probably have compressed the extent of the economic differences he assumes. An analysis of households over time might well have shown substantial mobility and little social differentiation, if any.

To conclude, this is a work of limited value, providing a future researcher with a useful discussion of the available sources and other historians with a standard, almost clichéd, Soviet view of social and economic developments in rural Russia after 1861.

STEVEN HOCH
Drew University

R. G. EIMONTOVA. *Russkie universitety na grani dvukh epokh: Ot Rossii krepostnoi k Rossii kapitalisticheskoi* [Russian Universities on the Border of Two Epochs: From Serfdom to Capitalism]. Moscow: Nauka. 1985. Pp. 345. 2r. 50 k.

R. G. Eimontova has published a number of articles and chapter-length studies of the university reform of 1863 and its place in the era of "Great Reforms." The present book does not take up the statute of 1863, since, as the author explains, she has done that elsewhere and also plans another book devoted specifically to it. This book focuses on the professors and students in Russian universities in the 1850s and 1860s. An introductory chapter describes the theme and discusses the literature and sources. A chapter on the "dark" period, 1848–54, is followed by a chapter on the professors and one on the students. A brief conclusion is the final chapter.

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Her dissertation and article entitled "The University Reform of 1863" (*Istoricheskie zapiski* [1961]) demonstrated Eimontova's complete command of the rich prerevolutionary literature on the subject. Since that time Western scholars have contributed important studies of the era of the Great Reforms and specifically of the universities. Eimontova ignores them all, noting only that a "foreign bourgeois" literature exists, marked by "frequently distorted interpretations" (p. 7). Her brief discussion of literature and sources, that is, prerevolutionary Russian scholarship, Soviet contributions, and the published and archival sources, makes no significant advance over the state of the question in 1961. Eimontova faults prerevolutionary Russian scholars for stressing the "so-called Western influence" at work in the university reform (p. 11). Quoting extensively from Lenin's many pronouncements on the subject, Eimontova aims to provide the "concrete" details that show how the objective requirements of the transition from feudalism to capitalism, the fundamental process at work in the Great Reforms, promoted certain changes in the universities. The universities changed in three significant ways: the class origins, level of academic accomplishment, and ideology espoused by faculty and students; the character and content of teaching; and the order and style of university governance.

In her conclusion Eimontova, not quoting Lenin, says that the changes in the universities proved important because the universities formed one center (the press was the other) that developed "critical thinking in society, popularized bourgeois-democratic ideas that were progressive in the Russia of that time," and "played a role in spreading constitutional ideas," for "teachers and students formed the heart of the liberal movement" (p. 326). This conclusion's echoes of prerevolutionary liberal scholarship are well supported by the cases developed in the chapters on the professors and students. The chapter on the "dark" period, 1848–55, describes the universities from their foundation in the first decade of the century. The discussion, unfortunately, is thin, retelling the story of tsarist reaction without benefit of much recent work on such figures as S. S. Uvarov. The chapters on professors and students are carefully done and richly documented. Occasionally the reader might wish for more explanation. For example, although most university faculties wanted to admit female students, the Moscow faculty voted against it. Eimontova establishes the fact but makes no effort to explain why the Moscow faculty was so out of step with fellow liberals in this matter.

"Not accidentally," as the author says of many of the points made, she tends to match the evidence to the explanatory scheme outlined in the first chapter. Nonetheless, along the way Eimontova provides a detailed and sympathetic account of the faculties' pursuit of self-governance, of the student attempts to gain rights that obtained in "West European and Dorpat universities," (p. 258) of the important roles played by Poles in the struggle for freedom, of the importance of the government's cooperation in launching many liberal steps, of the great importance of spreading Western science and political and economic ideas, and much else. Discussing the relationships between the conservative "fathers" and the liberal "sons" of the Moscow faculty, Eimontova notes that "in reality, the situation was more complicated" than any simple explanation might allow (p. 229). Her book facilitates appreciation of many of the complexities of the history of the Russian universities in the mid-nineteenth century.

JAMES T. FLYNN
College of the Holy Cross

PREDRAG M. GRUJIĆ. *Čičerin, Plechanov und Lenin: Studien zur Geschichte des Hegelianismus in Russland.* (Sozialphilosophische Studien, number 5.) Munich: Wilhelm Fink. 1985. Pp. 290.

In 1969 Predrag M. Grujić published a study entitled *Hegels Einfluss auf die Sowjetphilosophie der Gegenwart.* Now, after completing other works on Marxist philosophy, he returns to the theme of Hegel and Russian Marxism by addressing himself to the German philosopher's impact during an earlier period—specifically, to his influence on Boris Nikolaevich Chicherin (1828–1904), Georgii Valentinovich Plekhanov (1856–1918), and Vladimir Il'ich Lenin (1870–1924).

At first glance, Chicherin seems out of place in this trio. One-time mayor of Moscow and lifelong advocate of constitutional monarchy, Chicherin was no Marxist. But he was without question a Hegelian—the greatest of the Russian Hegelians, Grujić believes—who in his many philosophical works sought both to do justice to his German master and to develop the latter's intellectual legacy creatively. As such, Chicherin represents for Grujić an early stage of the sustained Russian effort to transcend Hegel sympathetically—the effort that eventually produced Russian dialectical materialism.

The author expounds Chicherin's philosophical system in detail and devotes twice as much space (one hundred ten pages) to it as to the thought of either of his two other subjects. He argues that Chicherin sought above all to bring the dialectical philosophy of Hegel down to earth by investing it with greater empirical content and practical relevance. But, Grujić believes, Chicherin was too close to Hegel to succeed in this endeavor, and he remained an idealist.

In Plekhanov's work, the author contends, the attempt to concretize the Hegelian system is contin-

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ned in more thoroughgoing fashion. It takes the form of Plekhanov's version of the materialist theory of history, which, according to Grujić, stems from Plekhanov's interest in the Hegelian dialectic and in Feuerbach's critique of that dialectic and reveals its Hegelian origins in such features as the dialectical connection of freedom and necessity. Thus, the author argues that, in Russia, the materialist theory of history—the very heart of "Marxist" thought—was initially structured with an eye more to Hegel and Feuerbach than to Marx or Engels.

Lenin's departure from the idealism of Hegel is more radical still and is not confined to the theory of history, Grujić asserts. Lenin is said to have effected the universal materialization of the dialectic through the medium of the Hegelian view that the same dialectic operates in both the subjective and the objective worlds. Lenin's special contribution to Marxist philosophy, according to Grujić, consists in his thesis of the unity of dialectics, logic, and the theory of knowledge—a thesis drawn from Hegel's *Logic*, in which the logic of thought and the logic of being are one. The author contends that we must interpret Lenin's "theory of reflection" in this context: the theory is traceable not to Engels but to Hegel. Lenin, by his materialist identification of the subjective and objective dialectics, Grujić argues, succeeded where Chicherin failed: he gave practical and empirical significance to Hegelian philosophy.

By implication, Grujić accepts the intellectual development from Chicherin through Plekhanov to Lenin as a case of philosophical progress, and he accepts Lenin as an important and original philosopher—theses that are, at the very least, controversial. But his real interests lie not in championing particular philosophers or pleading a philosophical case but in tracing the impact of Hegelian ideas on Russian Marxism, and he does that with admirable expertise and with considerable if not always complete plausibility. Even those who have reservations about his argument, however, will be grateful to him for presenting the fullest treatment of the philosophy of Chicherin in any language, for detailing in three key instances the evidence of Hegel's influence on Russian thought, and for forcefully questioning the influence of Marx and Engels. It is always useful to be reminded what little Marx there is in Russian Marxism.

JAMES P. SCANLAN
Ohio State University

N. B. LOSINSKII. *Revoliutsionno-narodnicheskoe dvizhenie v Belorussii, 1870–1884 gg.* [The Revolutionary-Populist Movement in Belorussia, 1870–84]. Edited by A. I. KOZHUSHKOV. Minsk: Nauka i Tekhnika. 1983. Pp. 222. 1 r. 60 k.

N. B. Losinskii's main purpose is to show that Belorussian revolutionary-populism (*narodnichestvo*) was "closely associated with the movement everywhere else in the empire and organizationally became a component of it" (p. 16). Belorussian *narodnichestvo* was neither local nor distinct in character. "It combined with and replicated the main course of Russian history, demonstrating the *zakonomernost'* [coherence, process, governance by systematic laws] of its own failure and the victory of Marxism-Leninism" (pp. 189–90).

The history of *narodnichestvo* at the all-Russian level, we learn, has been told well enough. The provincial and local histories, however, have been less well done. Losinskii corrects prerevolutionary "false notions" about a distinct Jewish movement in Belorussia and chastises two leading Soviet specialists. S. M. Sambuk's *Revoliutsionnye narodniki Belorussii (70-e–nachalo 80-kh godov XIX v.)* (1972) was based on new materials but placed too much emphasis on the national particularity of Belorussia. T. G. Snytko's *Russkoe narodnichestvo i pol'skoe obshchestvennoe dvizhenie, 1865–1881 gg.* (1969) placed too much emphasis on Poland (pp. 47–56). Certain West German *Ostforscher* and Belorussian nationalists have jumped into this breach to argue that Bolshevism had no native roots in the local revolutionary heritage (p. 10). These miscreants remain unnamed, but footnotes refer to vague refutations of "bourgeois" historians.

Losinskii himself cannot ignore the particularities of Belorussia, and one wonders finally if his position is really much different from Sambuk's and Snytko's. He concedes in a footnote that Poles and Jews did after all constitute between 20 and 24 percent of the population of Minsk province in 1873. Furthermore, he gets all twisted up in a misleading particularity of Belorussia when he emphasizes that Minsk, Grodno, Mogilev, and Vitebsk provinces were free of the fabled peasant commune (*obshchina*). Working from a standard cliché image of *narodnichestvo*, Losinskii says that "consequently it was not the utopian *obshchina* projects of the narodniks but their militant revolutionary democratism that attracted the *raznochintsy* intelligentsia of Belorussia to the ranks of the populist movement" (p. 187). Belorussian narodniks concentrated on workers rather than peasants, the writings of Marx and Engels came into wide usage there, and the city of Minsk became one of the hotbeds of emerging social democracy in Russia. Just how different all this is from other areas is not demonstrated, it is just asserted. One reference to any other regional study, for example, to V. N. Ginev's *Narodnicheskoe dvizhenie v srednem povolzh'e, 70–e gody XIX veka* (1966), might well have been substituted for any one of the fifty references to Lenin.

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Using materials in Russian, Belorussian, and Polish, Losinskii brings to his work extensive and detailed research in local and national archives and libraries. He has compiled a file of data that contains 469 "people who participated in the Belorussian narodnik movement, 1870–1884." These data are subjected to aggregate statistical analysis in eleven tables (pp. 192–200). They are combined with other more traditional narrative evidence to define the "mistakes, inadequacies, and errors" (p. 182) of Belorussian *narodnichestvo* and to demonstrate that these were, on the whole, characteristic of the movement in other regions. Belorussian populists lacked firm ties with the people. Only about 8 percent were peasants; about 50 percent were from the middling elements of society (*meschchane*) (p. 194). Only 7 of 469 were indicted for propaganda among the peasantry, and only 26 for such activity among workers. Compare those figures with the 148 indicted simply for membership in or association with discussion circles or other organizations (p. 198).

Who are these people, really? Are they all "populist" revolutionists? Of the 469 arrested or otherwise indicted, only 3 were executed, 6 sentenced to hard labor, 73 exiled in Siberia or the northern regions of European Russia. This means that only 82 were punished with any severity at all. Seventy-six went into hiding or emigrated. Of the remainder, 171 were simply put under surveillance, 75 temporarily confined under arrest, and 65 sent home to Belorussia from central provinces (pp. 198–99). Only one out of five activists was more than twenty-five years old. Thirty-nine percent were students in secondary schools (pp. 193–95). Do these data measure a revolutionary movement or police practices? Aggregate statistics do not answer these crucial questions without complementary microanalysis of specific groups and clear comparison with comparable data for the whole empire.

ALAN KIMBALL
University of Oregon

DONALD J. RALEIGH. *Revolution on the Volga: 1917 in Saratov.* (Studies in Soviet History and Society.) Ithaca: Cornell University Press. 1986. Pp. 373. $32.50.

Historical research in the West on the Russian revolution has made considerable progress in recent years, particularly in the analysis of the social forces that propelled the revolutionary process in 1917. Despite this progress, however, the study of the revolution in local areas has been neglected. Donald J. Raleigh has now produced a major monograph to fill this gap.

Saratov is a perfect place to examine provincial revolutionary dynamics. The city of Saratov and nearby Tsaritsyn were two major industrial centers in the lower Volga with a history of working-class movements. In addition, a large contingent of soldiers was stationed in the province as well as in the city of Saratov. Also, the countryside of the province had been a hotbed of agrarian disturbances; its former governor Petr Stolypin and influential Social Revolutionaries (SRs) had competed to change the village according to their own image. Raleigh superbly analyzes the interactions between the political process and these diverse social movements in the "depths of Russia" from the February revolution until a few months after the Bolshevik assumption of power.

The revolutionary process in Saratov basically paralleled that in the capitals. Raleigh's analysis does not fundamentally change our understanding of this process. But, within this basic framework, his study reinforces some of our previous assumptions, revises others, and raises important new questions that have not been examined fully.

Most importantly, Raleigh clearly demonstrates that the revolutionary process in Saratov was not a mere echo of the events in Petrograd but rather a violent revolutionary process of its own, which, although sensitively reacting to the events in the capital, followed the logic of local conditions. That this process took a similar pattern to that in Petrograd demonstrates the depth of social chasm between the upper and lower classes that pervaded the remotest corner of Russian society.

But, unlike the socialists of Petrograd, the socialist leaders in Saratov participated in and took the leadership of the Public Executive Committee, while the Saratov Soviet was also formed independently. Here Raleigh makes the important point that in this case Petrograd was an exception. In many other cities a public (*obshchestvennyi*) organ based on the principle of equality of all classes was formed with the cooperation of socialist leaders. As social divisions began to manifest themselves, this principle had to give way to the class principle of soviet democracy to the exclusion of propertied (*tsenzovoe*) society. Once class hostilities came to the fore, liberalism had little chance for survival, and the moderate socialists, who clung to cooperation with the bourgeoisie to the last moment, had to be washed away by the explosive forces from below clamoring for soviet power.

Raleigh also argues that the most important result of the February revolution was not so much new institutions as "an entirely different approach to politics" (p. 75). Centralized control was rejected, and, instead, the notion of autonomy became prevalent at all political levels. Thus, the Provisional Government's nomination of a new governor was overruled by the Public Executive Committee. Within Saratov itself lower-level organizations based

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on factories, military units, and villages asserted themselves against the will of the Public Executive Committee, the Military Committee, and even the Saratov Soviet. Thus, in Saratov, the Provisional Government and the Petrograd Soviet were almost irrelevant in determining the actual course of events.

The image of the Bolshevik party as a monolithic organization is convincingly destroyed by Raleigh's analysis. The Saratov Bolsheviks consistently acted independently as shown by their strong tendency to cooperate with the moderate socialists before April and by their adherence to the slogan: "All Power to the Soviets," even after the Sixth Party Congress. Raleigh's findings reinforce, from a different angle, Alexander Rabinowitch's interpretation of the Bolshevik party in Petrograd.

Raleigh makes clear that the Bolshevik seizure of power was not an accident limited only to Petrograd and the northern provinces. Already in September Tsaritsyn was virtually in the hands of the Bolsheviks, and the Saratov Soviet was controlled by the Bolshevik majority. But the author also stresses the importance of Left-SR support for the maintenance of Soviet power after the Bolshevik seizure of power, particularly in the rural areas.

The kaleidoscope of intricate interactions betwen politics and social movements, between national and local politics, between cities and countryside, and between the moderate and radical socialists is skillfully described. Even the rising crime rate, deteriorating housing and health conditions, unemployment, and brutal mob justice, which gradually reduced Saratov to anomie, do not escape the sharp eyes of the historian. The author should be commended for successfully combining scholarly analysis with a style that vividly captures the excitement of the local revolution.

This book, based on a massive array of published sources, demonstrates that a good monograph can be written even without access to archives. Raleigh's work should thus encourage specialists outside the Soviet Union. Raleigh has written a major work that should be considered one of the classics on the Russian revolution. The excellence of this work is superseded only by the ingratitude of the Soviet authorities, who have not permitted the historian who has contributed more than anybody else to the understanding of the revolutionary heritage of Saratov to visit the birthplace of the revolution on the Volga.

TSUYOSHI HASEGAWA
Hokkaido University

Z. L. SEREBRIAKOVA. *Regional'nye sovety natsional'nykh raionov, mart 1917–mart 1918* [The Regional Soviets of the National Areas from March 1917 to March 1918]. Moscow: Nauka. 1984. Pp. 196. 1 r. 90 k.

An important and fruitful trend in recent Western and Soviet studies of the Russian revolution has been toward in-depth analysis of the various groups, classes, organizations, and institutions that helped shape the revolutionary process. In the last few years an impressive array of excellent monographs have been published on the peasantry, the working class, the military, the main political parties, a few of the national minorities, and some of the mass organizations that sprang up spontaneously in the heady days of 1917. Gaps in the coverage nonetheless remain, and one subject that merits more attention from historians is the development of the soviets—especially on Russia's multinational periphery.

Judged by its title and introductory chapter, Z. L. Serebriakova's study of regional soviets in Latvia, Estonia, Belorussia, and the southern Ukraine appears to be a promising venture into relatively unexplored territory. Unfortunately, the book fails to fulfill this promise. Superficial and inspired more by ideology than scholarship, it falls well short of the standards set by A. M. Andreev's *Sovety rabochikh i soldatskikh deputatov nakanune oktiabria* (1967) and other serious Soviet works on revolutionary organizations. Serebriakova's simple, unremarkable thesis is that the emergence of regional soviets following the February revolution gave the national minorities an early opportunity to create an organizational basis for national self-determination within the framework of a Soviet Russia. She further asserts that only the Bolsheviks, guided by Leninist internationalism and "well acquainted with the national peculiarities of the regions" (p. 175), could ensure the realization of the regional soviets' potential. Hence, in Latvia, where the Bolsheviks were powerful, the regional soviet quickly became an instrument of democratic self-determination and a bulwark against bourgeois nationalist efforts to sever the country from revolutionary Russia. By contrast, the regional soviet that embraced Odessa, the Black Sea Fleet, and the Romanian front (Rumcherod) was less successful mainly because it was dominated by the "petit bourgeois" Mensheviks and Socialist Revolutionaries.

The real misfortune of this book, however, lies neither in its simplistic, distorted analysis nor in its leaden, jargonistic prose but rather in its lack of new material. Although the footnotes indicate use of some archival sources, Serebriakova has based her account mostly on published documents and secondary works. Worse still, she avoids or skims over several important issues. Her discussion of relations between regional and local soviets is confusing and frustratingly incomplete, and she virtually ignores interethnic relations within the regions. One wonders, for example, how representatives of largely Russian military formations interacted with non-Russian locals. The book even fails to provide any

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information on the ethnic composition of the regional soviets. The history of the soviets in the western borderlands still awaits serious scholarly investigation.

ANDERS HENRIKSSON
Shepherd College

STEPHAN MERL. *Die Anfänge der Kollektivierung in der Übergang zur staatlichen Reglementierung der Produktions- und Marktbeziehungen im Dorf (1928–1930).* (Veröffentlichungen des Osteuropa-Instituts München, Geschichte, number 52.) Wiesbaden: Otto Harrassowitz. 1985. Pp. 443. DM 142.

This impressive study begins with the question, "Was it really necessary at the end of the 1920s to collectivize agriculture?" Fortunately, Stephan Merl does not consistently pursue that unpromising line of inquiry and instead concentrates on the measures that led to collectivization. He elucidates the contradictory, self-defeating nature of official decisions and returns to his initial question at the end. His reasonable conclusion is that collectivization answered neither the economic nor the ideological suppositions of a Leninist party but was the result of ignorance, obstinacy, and ruinous self-will by the later Bolsheviks.

Merl is enormously persuasive in showing that measures directed against well-to-do peasants (kulaks) redounded against the poor peasants (*bednota*); the *bednota* was economically dependent on the richer peasants who rented its otherwise unusable lands and provided it with employment. However miserable the wages may have been, the poor peasants could not do without them. As, in response to the discriminatory and draconian policies adopted in the wake of the "grain crisis" of 1927–28, the rich peasants divested themselves of livestock, equipment, and land, the *bednota* lost an important source of income and was driven into almost the same degree of desperation as those identified as kulaks.

The mass peasant movement toward collective farms in 1928 and 1929, in so far as it existed, was fueled by these poor peasants forced to seek some alternative to their dependency on, and exploitation by, the kulaks. But those dwarf collective farms, based on a few destitute members, were economically unviable and rested on the supposition of state support. By definition, poor peasants lacked livestock and equipment and they expected, on the basis of promises made by the party, to receive advanced agricultural machinery from the state. The state did not have the machinery to grant. In any case, machinery could not have been effectively applied within, nor would it have been granted to, the "dwarves." Thus, as the state increased pressure on the "kulaks," the *bednota* increasingly depended on the state, which, in turn, placed greater pressure on the middle peasant to pay for the establishment of collective farms.

Yet Merl, in his search for rationality, overlooks the possibility that the regime really was interested in promoting the interests of the *bednota*. However debased, politicized, and demoralized the Stalinist concept of class struggle had become (Merl is scrupulous in avoiding eponymous designations), the regime had a defensible rationale for promoting the interests of the *bednota*. It was, to be sure, secondary to the commitment to industrialization. And, ultimately, the regime washed its hands of the whole business and inflicted the same misery on both "kulaks" and *bednota* with marvelous impartiality. Still, the regime did not set out with that intention. Much to everyone's surprise, the social policy of the government, derived from Bukharinite postulates, backfired. Stalin's first instinct seems to have been not to force any issue but, once involved, to push on regardless of cost or, it seems, of sense: he was a bad poker player. Stalin plays a minor role in the drama Merl describes. The tragedy of collectivization, in the classical sense of good intentions leading to ruin, was not exclusively, perhaps not even mainly, Stalin's, although he provided the major justification for it, was the personal beneficiary of it, and was thereafter hagridden by it.

The old notion that collectivization materially contributed to "primitive socialist accumulation" has long since been exploded. The costs of collectivization in the 1930s far exceeded its economic benefits, and Merl demonstrates that the initial steps toward it were equally detrimental. One might imagine that, from the party's perspective, each move made the next more imperative; Merl does not hold this view. For him each step is a further indication of madness and a point at which the process might have been stopped, reversed, and made sane. For him the whole process was an exercise in irrationality. So it proved to be, but collectivization rested on a logic that is not examined here.

As the regime pushed for collective farms, peasants disposed of their means of production, especially livestock and complex machinery, and thus deprived the "cooperative farms" of the means for their existence. That deprivation lent greater inducement to the bosses to up the ante for admission to the collectives—to institute a greater degree of "socialization" of the means of production, which amounted to the expropriation of the property of the middle peasant. But expropriation was self-defeating, too, for the equipment of the middle peasant was simply not applicable to the "giant" collectives, which were created supposedly as a re-

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sult of the industrialization of agriculture through "machinization," which would not occur until two generations later.

Merl is especially interesting in demonstrating that the very lack of agricultural machinery, which was presumed to be the basis of collectivization, became the spur to it. The bureaucratic struggle for what little equipment existed led to higher and higher targets of collectivization proposed by the cooperative movement, the collection agencies, and the tractor centers without any evident regard for peasants' needs or preparation to participate: each bureaucratic entity sought to protect and promote its clientele in the eyes of the regime. Again, the result was disaster.

Altogether, despite its wrong-headed initial question, this is a very important book, a valuable increment to the works of V. P. Danilov, M. Lewin, T. Shanin, R. W. Davies, and others. This review is too short to pursue the many suggestive notions and extremely rich materials (105 tables, almost all of them pertinent) given here.

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LAWRENCE BADASH. *Kapitza, Rutherford, and the Kremlin*. New Haven: Yale University Press. 1985. Pp. xi, 129. $20.00.

MARK POPOVSKY. *The Vavilov Affair*. Foreword by ANDREI SAKHAROV. Hamden, Conn.: Archon. 1984. Pp. viii, 216. $19.50.

Both books under review here focus on the relationship between science and the Soviet state, primarily in the 1930s. Although neither volume fundamentally changes our conception of that relationship, both books add fascinating new details and glimpses of the personalities and interests involved. Lawrence Badash's volume is the shorter, consisting of an introduction and an edition of excerpts from letters written in 1934 and 1935 by the famous Soviet scientist, Peter Kapitza. The letters were written to his wife in England, after he had been detained in the Soviet Union during a visit to Leningrad in the summer of 1934. At that time he was informed that he would not be permitted to return to Cambridge, where he headed an important physics lab under the direction of Lord Ernest Rutherford. The excerpts published in this volume were provided by Kapitza's wife to Rutherford to keep him informed of her husband's state of mind. They were later found by Badash in the Rutherford papers in Cambridge but include only about one-fourth of the full text of Kapitza's correspondence with his wife in this period, which apparently was not available to Badash. Even these abbreviated texts, however, are sufficiently outspoken to explain why the volume was not published until after Kapitza's death in 1984.

Although not intended as a full biography, Badash's book tells us more about the enigmatic Kapitza's career and scientific work than any previous work in English. The son of a tsarist general, Kapitza was a brilliant experimentalist who developed complex machines to support Rutherford's research on atomic structure. After losing his first wife and two children to an influenza epidemic in Petrograd, he went abroad and settled in England in 1921, where he earned Rutherford's respect for his engineering talents and knowledge of physics. Kapitza developed a way of creating magnetic fields of great intensity, essential for nuclear research, patented an important method for the production of liquid helium in large amounts, and later developed a means for the production of low-cost liquid oxygen for use in industry.

Scarcely a reticent or modest man, Kapitza boasted to Soviet engineers of his achievements on his frequent trips to the Soviet Union during the late 1920s and early 1930s. Retaining his Soviet citizenship out of patriotism and general sympathy with the economic and social goals of the Soviet regime, Kapitza was told in 1934 that his talents were needed in the Soviet Union and that he would be made director of an important new institute but would not be allowed to travel abroad. Much of this book deals with the attempts by Rutherford and the British to attain his release and, when that failed, to help him get reestablished scientifically in the Soviet Union.

Kapitza comes across in these letters as sincere, outspoken, idealistic, a bit naive, and more than a little cantankerous. The letters provide a trenchant, insider's look at Soviet science, which Kapitza regarded as primarily imitative of Western science rather than as original or creative. His many portraits of scientific and bureaucratic personalities and the various absurdities of a state that needed science but distrusted scientists ring true. Badash's account is generally clear and accurate but would have benefited from a closer reading by specialists in Soviet history and the history of Russian science, which are not the author's specialties. For example, Badash sees Kapitza as unusual among Soviet scientists in his independence of mind during the 1930s. This view needs some qualification. The Soviet government had a high degree of tolerance for outspoken scientists if such persons were considered valuable. Examples of such outspoken scientists include I. P. Pavlov, V. I. Veradsky, D. N. Prianishnikov, and O. A. Ermansky.

Badash repeats the cliché that Russian science was strong in theoretical but weak in applied science

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before the revolutions of 1917—a view that more recent scholarship has significantly modified. He also errs in characterizing the Russian Academy of Sciences before 1927 as an unreformed tsarist institution. Actually, the academy, which was dominated in the late tsarist era by liberal opponents of autocracy, underwent significant liberalizing and democratizing reforms between 1918 and 1920. These points aside, the author has made a valuable and interesting contribution to the history of science and of Soviet society in a crucial era.

Mark Popovsky's volume on Nikolai Vavilov adds valuable new information to our understanding of that outstanding plant geneticist and his destruction by the Lamarkian agronomist Trofim Lysenko, whom Popovsky characterizes as the "Rasputin of Soviet agronomy" (p. 102), and his ally, the philosopher I. I. Prezent. Popovsky, who claims to have had access to the NKVD file on Vavilov while a trusted member of the Soviet establishment (before his emigration to the West in 1977), also cities interviews made between 1965 and 1977 with more than one hundred survivors of the Vavilov affair in the Soviet Union. The upshot of these new data is a significant revision of David Joravsky's book, *The Lysenko Affair* (1970). Joravsky, who did not have access to Soviet archives, concluded that, although many questions about the Lysenko affair remained unanswered, "Any way one searches it, the public record simply will not support the common belief that the apparatus of terror consciously and consistently worked with the Lysenkoites to promote their cause" (p. 122).

Popovsky sets out to prove precisely what Joravsky is most sceptical about: that Lysenko and Prezent, working hand in glove with the political police, prepared the ground for Vavilov's arrest and suppressed his scientific school. Although Popovsky's account cannot be corroborated in full until Western researchers are given access to the relevant Soviet archival sources, his argument on this point makes sense. Stalin, after all, had made it clear in the 1930s that he preferred the "barefoot scientist" Lysenko, who seemed to have a set of panaceas for critical agricultural problems, whereas scientists from bourgeois backgrounds such as Vavilov hemmed and hawed. Stalin disliked the erudite Vavilov, the son of a liberal, self-made millionaire. While Lysenko was putting forward panaceas for practical problems, false though those solutions turned out to be, the cautious Vavilov was traveling around the world looking for the historical origins of edible plants and gathering one of the world's largest collections of seeds, an expensive endeavor whose practical uses were not clear to political leaders such as Stalin. Vavilov's scientifically valuable work seemed to produce few practical results until his experiments with hybrid corn in 1937. By then the Lysenkoites were too powerful and the Stalinists too sceptical of Vavilov and his followers for them to gain the general secretary's confidence. Vavilov was arrested for sabotage in 1940, the victim of a systematic campaign of denunciation by Lysenko and his followers, according to Popovsky. Despite the efforts of some courageous scientists to save him, Vavilov died of malnutrition in a Soviet prison in 1943. His reputation was revived in the Soviet Union only after 1965, with the fall of Lysenko. More than twenty-five hundred pages of Vavilov's unpublished manuscripts disappeared with his arrest and have never resurfaced, including a thousand-page book entitled "Combating Plant Diseases by Breeding Resistant Varieties," surely an important loss to world science.

Popovsky's book points out but does not clear up several other mysteries about Vavilov. For example, Popovsky cites abundant evidence to indicate that for many years after their first acquaintance in 1925 Vavilov was one of Lysenko's strongest supporters and helped sponsor his rise from obscurity. Even when it became clear in the mid-1930s that Lysenko and Prezent were intent on his destruction, Vavilov held back from an open confrontation with the Lysenkoites until 1939, when it was already too late. As Popovsky puts it, "It is quite clear from the documents that in the early 1930s academician Vavilov opened the way for Lysenko to make a career in government service and in science."

Not Vavilov but other biologists such as Mark Zavadovsky of Moscow University in 1935 and three plant breeders—P. N. Konstantivov, P. I. Lisitsyn, and A. N. Shekhurdin—in 1936 took on Lysenko and tried to prove that his methods were valueless. Why did Vavilov, who commanded a large experimental apparatus and an army of researchers, apparently never subject Lysenko's wild claims to the test of experiment? Popovsky would have us believe, on the testimony of friends of Vavilov, that his naiveté and childlike trust of others prevented him from attacking Lysenkoism until the late 1930s. But, if so, such trustfulness casts doubt on Vavilov's competence as a scientist and is in contradiction with one of Popovsky's other aims: to rehabilitate Vavilov as an outstanding plant scientist and geneticist. As late as 1935 Vavilov was still defending Lysenko from his detractors before the presidium of the Academy of Agricultural Sciences with such statements as the following: "Lysenko is a careful and talented reseacher and his experimental work is irreproachable." Popovsky points this out but fails to explain how such an outstanding scientist as Vavilov could be taken in for so many years by a charlatan.

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NEAR EAST

AMOS PERLMUTTER. *Israel: The Partitioned State; A Political History since 1900*. New York: Charles Scribner's Sons. 1985. Pp. xiv, 398. $19.95.

This book reviews Israel's political history, from the prestate days of the *Yishuv* right up to the 1984 elections that resulted in the Peres-Shamir coalition. The book relies heavily on secondary works (especially in the first half, which deals with the prestate period). But even in the later chapters, which deal with the Begin administrations and the Lebanon war, Amos Perlmutter's connections with the Israeli military and political elite ("my own personal and sometimes intimate acquaintance with practically all of the significant political and intellectual leaders of Israel since the 1960s" [p. xiv]) produce little that has not been revealed already by Ehud Yaari and Zeev Schiff.

The author, who has established a reputation as a political scientist with an insight into Israeli domestic politics, informs us that the best analysis of relations between Ben-Gurion and the Israeli army remains his own. Perlmutter claims to have used a "multitude of original documentation . . . and secondary literature in Hebrew, English, German, Yiddish and Arabic" (p. xiii). The bibliography, however, contains but two entries in Arabic, none in German or Yiddish, and no citations in those three languages appear in the footnotes.

Perlmutter's declared goal is to place the complex contemporary scene in Israel into its "historical context." That "context" is then coerced into the single conceptual framework of "partition." We are informed that "the whole political history of Israel is the history and evolution of the partitioned state" (p. 12). The pre-1948 partition plans are seen as the work of foreign powers and subsequent plans as the result "of aggressive wars initiated by the Arabs against the Jews" (p. 7). Quite obviously, the author's theoretical framework does not quite fit the facts. Neither the 1956 nor the 1973 war resulted in any "partition," and, if anything, the 1967 war undid the 1948–49 partitions. Even on this central issue Perlmutter confuses the facts. On page 6, he refers to the three partitions of Palestine since the establishment of the mandate—in 1947, 1967, and 1973. But, of course, as the author himself tells us on page 12, Palestine was partitioned first in 1922, when the Churchill White Paper excluded Transjordan from the terms of the Balfour Declaration.

The strength of this book, if any, lies in the author's intimate acquaintance with the Israeli political scene. Of particular interest is his analysis of the so-called Lavon affair (a bungled intelligence operation in 1953) and its domestic political repercussions; the political demise of Ben-Gurion in 1965 and, after the Yom Kippur war, the end of Labour hegemony. But here again, as happens all too often, the author tends to burden his analysis with an exaggerated emphasis on a single aspect of a complex train of events.

The book is replete with careless mistakes and typographical errors. Space permits just a few examples. The 1939 White Paper did not forbid all further immigration and land purchase (p. 12), although, again, the author himself corrects this (p. 39). Chaim Weizmann's talks with Faisal in 1919 were not a complete failure (p. 33); they resulted in a written agreement, even if that agreement was not eventually consummated. The Rashid Ali coup took place in 1941, not 1942 (p. 90). The "Sezon" (pp. 98–99), which the author notes was a term taken from French, should of course be *Saison* (hunting season). The commander of the *Exodus* was not Eliav (p. 244) but Ike Aranne. Yossi Sarid does not merit the epithet "opportunistic" (p. 245); he was the only man to oppose the Lebanon war consistently, from its first day. The 1947 partition was not the work of Britain and the United Nations (p. 265)—the British certainly cannot be accused of that! On page 119 we find: "America's policy [from 1946 to 1948] in fact was not nearly as much affected by the Jewish vote as has been assumed." Then, just seven lines below, Perlmutter writes that "the Zionists, on the other hand, in spite of the basically anti-Zionist attitude in Washington, were remarkably successful in manipulating American action."

Scholars acquainted with this field will find little new here. The lapses into "journalese" suggest that this may have been aimed at a wider, more popular audience.

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AFRICA

MICHAEL CROWDER, editor. *The Cambridge History of Africa*. Volume 8, *From* c. *1940 to* c. *1975*. New York: Cambridge University Press. 1984. Pp. xvi, 1011. $84.50.

The eighth and final volume of *The Cambridge History of Africa*, which outlines the history of Africa between World War II and 1975, reminds me of a Jacobean tragedy. At the final curtain all promises have been betrayed and shrouds are scattered about everywhere. This history is certainly no dry-as-dust recounting of facts. The facts have been woven, rather, into the tapestry of a story of how the nationalist movements of Africa, initially cloaked in so much promise and buoyed up by so many high expectations during the push toward decolonization

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in the 1940s and 1950s, guttered out in the realities of the 1960s and 1970s. The result of this failure has been an increasingly impoverished populace ruled over by oppressive, but hardly secure, elites chronically unable to cope with the problems their states have faced.

The volume is divided into two major sections. The first of these consists of five chapters that present synthetic interpretations of the principal historical processes that have shaped Africa since 1940. The initial chapter, by Michael Crowder, in an elegant, magisterial, and largely convincing interpretation, sets the stage by stressing the early political and social importance of the main actors of the story, the members of the new elite groups that had grown up in the 1920s and 1930s. For him, World War II gave them the opportunity to organize effective anticolonial nationalist organizations.

The second chapter, by the late Billy Dudley, focuses on the problems that the members of these elites faced at independence, most particularly their states' dependency on uncontrollable economic and political forces beyond Africa. Ian Duffield, in an extended analysis of the dismal failures of pan-Africanism—long on rhetoric and short on results—presents an implicit, and somewhat curious, counterfactual statement as to what might have been had the movement had greater potency. J. D. Y. Peel, in a clear and sensitive essay, assesses social and cultural changes that paralleled and were related to economic changes. Peel emphasizes growing class differentiation and the cardinal importance of the state as an object for intense political struggle. Finally, Adebayo Adedeji outlines economic changes that have occurred in the period. A great deal of information is included in this chapter, with many graphs and tables, and his conclusion is fully in keeping with the volume's pervasive melancholy: in spite "of all efforts by governments to accelerate the rate of progress, analysis of the economic evolution of Africa since 1940 has shown that no fundamental change took place after independence" (p. 248).

The second part of the book consists of ten chapters that discuss various zones of Africa. In this section the basic theme of the undermining of nationalism's promise by the gritty realities of political instability and economic impoverishment recurs and is fleshed out in specific case studies. Because the majority of the authors are not historians, political events and strategems are clearly the main areas of focus. Reflecting perhaps the authors' collective preoccupation with the educated African elite, the geographical areas are defined largely, although not entirely, according to the language of colonial occupation. David Williams and Cherry Gertzel deal with what the editors term "English-speaking" West and East Africa, respectively. "French-speaking tropical Africa" is handled by Ruth Schachter Morgenthau and Lucy Behrman, and Bonar Gow assesses Madagascar. Crawford Young attempts to bring some order to the historical morass of the former Belgian colonies, and Basil Davidson again relates his knowledge of the evils of Portuguese rule in Mozambique, Angola, and Guinea-Bissau. Equatorial Guinea, where, inconveniently for the editor, Spanish was the language of colonial control, has been ceded to Davidson as an embarrassing residuum. Christopher Clapham's brief is the Horn of Africa, and Hans-Heino Kopietz, Pamela Smith, and Clement H. Moore survey North Africa.

Francis Wilson's discussion of southern Africa was, for me, one of the most effective chapters. Wilson presents a mass of useful statistical data but weaves it into an interpretation that is marked both by occasional lyricism and by passionate—and quite appropriate—personal involvement. Wilson's view of southern Africa is as gloomy as that of the other authors', concluding that the area seems to "be moving inexorably towards massive conflict" (p. 330), and he sees civil war already in process.

To the student of contemporary Africa—whether interested in its history, politics, or problems of economic development—this book will be of great value. The skillfully written syntheses present much interesting data. Each chapter is supported by a useful bibliography or bibliographical essay, although scholarly work of the past several years is missing, for the volume was slow to be brought to press. Best of all, however, the volume's basic tone will engage readers whether they agree or disagree with the authors' points of view. The sense of tragedy that infuses the book cannot help but compel thought by the reader, and, as such, this volume will serve as an excellent point of departure for further studies.

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B. JEWSIEWICKI, editor. *Mode of Production: The Challenge of Africa/Mode de Production: Les défis africains.* Assisted by J. LETOURNEAU. Ste-Foy, Canada: Safi. 1985. Pp. ii, 174.

A separately produced number of the *Canadian Journal of African Studies*, this volume is devoted to the contribution of the concept of modes of production to the study of African political economy. Appropriately for its bilingual journal of origin and reflecting the contribution of French scholarship to the theory, definition, and application of modes of production to the African situation, some of the chapters are in French and some in English. Twenty-one authors reflect on the influence of the concept in their own development, present practice, and future agenda. On the whole, the volume serves

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as a memorial to an important but largely spent force in the historiography of Africa.

Bogumil Jewsiewicki, the principal editor and moving force behind the project, introduces the topic appropriately as about "a debate, a paradigm, and an epistemology" (p. 2). The debate has concerned the level of specificity on which to describe modes of production, and French anthropologists have led the way in disputes over whether production in the sense of occupational specializations or appropriation by lineage or community authorities is the hallmark of the phenomenon at the local level. At the commanding heights of the world system, on the other hand, others have contested the same basic question: whether capital is dominant in its merchant or industrial form and how this hegemony is manifested in accumulation. Each one of these key words is controversial, and the inability of scholars to come to agreement over their meaning has thwarted the progress of the paradigm and the epistemology.

The three sections of the book reflect different moods. In the first, on closure, one French, one South African, one English, and four North American historians appraise the concept retrospectively, with both appreciation and disenchantment. The second section comprises chapters by anthropologists and historians still committed to the use of the concept and demonstrates its persisting epistemological attraction. Lastly, the most theoretically inclined contributors, some of them veterans such as Emmanuel Terray and Pierre-Philippe Rey and some who are their successors, but none of whom is a historian, advocate "new starts" in light of the criticisms and experience that accrued in the 1970s.

Younger historians of Africa encountering the historiographic legacy of the mode of production as a debate, a paradigm, and an epistemology will find this volume useful. The challenges of understanding the complex changes in values and material conditions in the nineteenth and twentieth centuries must be met with some means of generalizing about the social distribution of wealth by governments from global, continental, and local perspectives. Like the infamous groundnut scheme in post–World War II Tanganyika, where in the spirit of a total campaign the British threw heavy machinery and other investments into a fragile and ill-considered environment with disastrous results, a heavy-handed theory will not advance productive scholarship about Africa. Nevertheless, as Gervase Clarence-Smith argues, a nondogmatic, heuristic use of models in describing production and appopriation will help build the wider empirical base in political economy and labor history that is presently much needed.

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D. ELWOOD DUNN and SVEND E. HOLSOE. *Historical Dictionary of Liberia*. (African Historical Dictionaries, number 38.) Metuchen, N.J.: Scarecrow. 1985. Pp. xx, 274. $22.50.

This book by D. Elwood Dunn and Svend E. Holsoe summarizes background data on over five hundred persons, places, institutions, and events in Liberia's history. Journalists, policy makers, and scholars of African and American history will find this a good reference tool for research. Entries are concise yet comprehensive and include information on subjects from the early 1800s to the most recent political actors. The bibliography, arranged by topics such as culture, history, and economics, covers forty-seven pages. It is the most accessible bibliography available for both generalists and Liberianists. Seven maps and an eight-page chronology are also included.

The maps, the chronology, and the introduction, however, have problems. The seven maps are not dated, and no map of current political divisions is provided. The chronology is centered on Monrovia, a common problem in Liberian historiography. Significant events are omitted from the chronology. The arrival of eastern groups such as the Kru and Glebo should be noted, as should the founding of the Afro-American settlements of Sinoe and Maryland. Significant military resistance to the Liberian government that occurred in 1915, 1918, and the 1930s is omitted.

The introduction has a serious coastal bias, glosses over the contradictions of Liberian history, and presents information in a vacuum. The focus on Afro-American immigrant settlements without reference to the presence of indigenous Liberian societies in the early 1800s implies that they arrived to find empty space. When the authors state that "the two groups that constitute the Liberian population—indigenous and repatriated—had each come to the Liberian area in search of liberty," they imply that both groups arrived at the same time (p. 7). This view is reminiscent of the erroneous official South African version of the settlement of Europeans.

The reasons given for conflicts between settlers and Africans avoid the reality of deception and imperialism that are central to Liberian history. The notion that early hostilities created "an aboriginal problem" should be in quotations in the text (p. 8). The authors' attempt to look on the bright side of settler policy distorts history. For example, in their divide-and-rule strategies, the settlers created and manipulated more intra-African conflicts than they mediated. In addition, settler cleavages were geographic as well as economic; Monrovia dominated the power structure to the detriment of the eastern settlements of Sinoe and Maryland.

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Lastly, whether the book is directed at Liberianists or generalists is unclear, because the introduction refers to individuals and organizations without explanation. Brief descriptions would make references to Juah Nimley or MOJA more useful to the non-Liberianist.

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ANNE HILTON. *The Kingdom of Kongo.* (Oxford Studies in African Affairs.) New York: Clarendon Press of Oxford University Press. 1985. Pp. xiii, 319. $45.00.

Anne Hilton has given us a dense, but lucid, account of the history of the Bakongo state from the late fifteenth century until the end of the seventeenth century. In addition a concluding chapter summarizes the major literature on the kingdom's history in the eighteenth and nineteenth centuries. The work follows a straightforward format, with a substantial introduction being followed by six chronologically arranged chapters, each focusing on a major era for the kingdom, from its foundation as a unitary state until its transformation into an agglomeration of princedoms drawn together by a common interest in commerce and recognizing a single sacred monarch. Hilton has carefully mined the large and diverse body of literature on Kongo preserved in numerous European archives and libraries.

A helpful introductory survey of the physical environment of the territories of the kingdom illustrates the clarity and carefulness of Hilton's work. This chapter is accompanied, as is the case throughout the book, by helpful maps. The introduction continues with a rich analysis of fundamental aspects of Kongo social structure and belief systems, which uses an effective mixture of twentieth-century anthropological studies (notably those of Wyatt MacGaffey) and sixteenth- and seventeenth-century texts. Hilton is particularly full and useful in her attention to the structures of belief, their relationship to basic social formations, and their interchanges over time. She convincingly argues that the dynamic of the relationship between the religious and political aspects of Kongo life in response to changing environmental factors—particularly economic ones—shapes the history of the kingdom itself.

In the first of the chronologically ordered chapters Hilton examines the origins and nature of the Kongo kingdom. In so doing she takes on a long line of speculative literature on the subject, without falling into any of its pitfalls. Basically, she identifies the development, by the fifteenth century, of chiefdoms among the Kikongo-speaking inhabitants of the lower Zaire based on participation in the supply of copper and on the control of interregional exchanges of commodities or of lands particularly rich in the production of more widely sought goods such as salt, cloth, or shell money. She does not determine whether the kingdom that arose at Mbanza Kongo was directly imported from the copper-producing regions in the north or was developed locally by indirect stimulation. Whichever was the case, she emphasizes the important development of relations between the long-established, landowning, matrilineally oriented clans (*kanda*) of the region and the emergent chiefdoms that were not based on clans and supported themselves from the profits of exchange and the redistribution of valuable goods.

With the arrival of the Portugese in the late fifteenth century a more conventional, political-diplomatic history becomes possible. From this point Hilton mines the documents to give the most detailed and convincing history yet of the Kongo kingdom and especially of the elite who created it and were sustained by it. She is able, given her organizing framework, to deal with the role of Roman Christianity in the development of the Mwissikongo (the ruling group of the kingdom). She also pays careful attention to regional and international developments in relation to the evolution of the Kongo monarchy. Her command of internal family and religious politics and of regional economic, political, and diplomatic dynamics is sure and clear.

The book is not only equipped with a full complement of maps, each inserted at a most useful point, but also contains several interesting appendixes and charts, notably a list of kings through the seventeenth century, two royal genealogies, and two schematic representations of general kinship terms and relationships. The notes are extensive and the bibliography full and well arranged. Unfortunately (and beyond the author's control I feel sure) a small, but significant, number of typographical errors in dates in the text could catch the unwary.

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JOHN SPENCER. *The Kenya African Union.* Boston: Routledge and Kegan Paul. 1985. Pp. 296. $45.50.

The history of nationalism in Kenya has always been shrouded in myth, first by settlers who denied its legitimacy, later by colonial governments that interpreted its militant phase as a pathological social disorder, and recently by conservative nationalists who inherited the colonial state by appropriating the symbols but not the content of the militant movement. The foundations for modern scholarship were laid by Carl Rosberg's and John Notting-

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ham's classic *Myth of "Mau Mau"* (1966), and many of the details of the late period have subsequently been sketched in by local studies and individual memoirs. But the early history has remained shadowy until this account by John Spencer.

Spencer's focus is on political organization and leadership, as Kenyans, largely Kikuyu, struggled to develop an effective voice to express their grievances against the colonial government from the 1920s through the 1940s. He identifies a number of important themes to account for the conservatism and ineffectiveness of that voice. The Kikuyu Association was founded in 1919 by chiefs and early mission adherents from the reserves in the aftermath of widespread alienation of land, but its local members were limited in the challenges they could mount against a colonial government that employed them and served their interests. Harry Thuku's East African Association (EAA), by contrast, emerged in Nairobi among self-employed people in opposition to the chiefs and through militant rhetoric sought to appeal to educated young men and ethnic associations elsewhere in Kenya. Thus, by the time of Thuku's arrest in 1922, a number of the polarities—between those employed by the government and the self-employed, the old and the young, conservatives and radicals, the countryside and the city, parochial and national interests, and between Kikuyu and other peoples—that were to divide the nationalist movement in the years to come were already established.

The Kikuyu Central Association (KCA) emerged from the EAA's ashes and from the economic opportunities of the postwar years, as educated, self-sufficient traders organized to protest government economic restrictions. The KCA remained a small, localized movement, however, until the issue of female circumcision catapulted it into a mass pan-Kikuyu movement for cultural survival in the late 1920s. By the early 1930s, however, Kikuyu were absorbed by the independent schools and churches movement, the KCA was riven by splits among the leadership, and the party was unable to develop either a broad national alliance or national issues. Its successor after the war, the Kenya African Union (KAU), sought to establish its legitimacy and influence with a reformist colonial government, but its hopes were dashed in a resurgence of settler power, and its moderation and ineffectiveness prevented it from building a popular constituency. Radicals emerged within KAU, engineered a takeover of the central committee in 1950, expanded oathing to a new generation of taxi drivers, criminals, and even prostitutes, and thus set off sporadic outbreaks of rural and urban violence. The declaration of the emergency in 1952 rapidly isolated the militants, however, while the government commenced the reforms, long advocated in vain by KAU and its predecessors, that would consolidate and ultimately transfer power to the conservatives.

Spencer's study as outlined above is a finely detailed internal study of early African politics. A number of individual political leaders and issues come to the fore, but Spencer assumes knowledge of the numerous excellent analyses of Kenyan political economy and so does not demonstrate the dynamics of local political action within the wider context of a settler colony. The transition from initial governmental support to the ultimate bannings of both the KCA and KAU is not explained, for example; we do not see why the moderate nationalism of the KCA and KAU had to be impotent and ineffective regardless of internal factors. Similarly, the rise of both rural and urban radicalism in the 1950s is inexplicable on purely internal grounds; we see how colonial governments coopted individual nationalists but not how the settler state influenced and crippled African political discourse. Nevertheless, Spencer provides the detailed account of early African politics that wider studies lack. He frequently demonstrates what others have merely conjectured regarding such important issues as Kenyatta's essential conservatism and opposition to "Mau Mau," the ascendency of urban radicals prior the emergency, and the deep divisions within Kikuyu society that readily enabled the British to isolate the radicals and coopt the conservatives—divisions that continue in Kenyan politics today. For these reasons as well as for Spencer's extensive research, including an extraordinary number of interviews, this will remain the standard account of early nationalist politics in Kenya for some time.

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GEOFFREY WHEATCROFT. *The Randlords*. New York: Atheneum. 1985. Pp. xix, 314. $17.95.

Geoffrey Wheatcroft has attempted a huge task: to record the flamboyant personalities and endless maneuvers of South Africa's mining and financial entrepreneurs. Cecil Rhodes, Barney Barnato, Alfred Beit, J. B. Robinson, Julius Wernher, and others schemed and plotted to consolidate their holdings in Kimberley and then to establish a monopoly in the diamond trade. Wealth and power depended on limiting output and raising prices. The result was De Beers. Moving up to the gold fields, the Randlords had a different set of problems: increasing production at ever-deeper levels while cutting costs, which also called for centralization—the Chamber of Mines—and a steady increase in the supply of black labor at subsistence wages. It also required the Boer War and political control. The outcome was the South African state, a ruthless

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exploitation of African labor, and some enormous private fortunes that launched the Randlords (many of them eccentrics) and their heirs on the British political and social scene.

The strength of Wheatcroft's study is that it presents Johannesburg and the mines as the crucible of South African politics, the arena in which capital confronted the Boer republics, Afrikaner nationalism reacted to British imperialism, and the African majority was drawn into an industrial revolution. The study's weakness is that the author at times gets trapped in detail, lost in a welter of intriguing personalities. But, as competition, mergers, takeovers, and collusion produced more centralized structures, the tale is more clearly told. This South African story is a microcosm of the inherent tendency within mature capitalism to concentrations of corporate power—a reminder that J. A. Hobson, Karl Marx, and Joseph Schumpeter diagnosed the forces that would produce today's transnational giants. South Africa's variant is Ernest Oppenheimers's Anglo-American Corporation, backed by the Chamber of Mines, which came to control the world the Randlords created.

Wheatcroft's approach suffers from additional shortcomings that should be mentioned. He cannot resist the occasional aside that reveals his limitations rather than shrewd judgment. Writing of Kruger's republic, he states that "the Transvaal was not really a 'state' at all, scarcely more so than the neighbouring African kingdoms or for that matter the present-day countries of independent Africa" (p. 78). Turning to the current South African scene and black protests, Wheatcroft suggests Africans "want to be rid of white supremacy and yet they want to go on enjoying the benefits of a rich industrial society. Soweto is not merely a grim compound: it is the most prosperous black African city anywhere. This explains the continuing failure of the African Nationalist Congress and other revolutionary organizations to stir up the blacks. They have compromised with gold" (p. 268). Such glib and misleading comments will not do. Apparently Wheatcroft knows little of traditional states or of those of tropical Africa; certainly he has little understanding of black political movements that will eventually control the legacy of Rhodes and his cohorts. In addition he has not given enough attention to the Marxist school of South African historiography.

In spite of these flaws, this book is an entertaining and, at times, groundbreaking survey of the range of capitalist entrepreneurs, swindlers, adventurers, and idealists who created the mining base of South Africa's industrial revolution. Wheatcroft has also given us a valuable and carefully annotated bibliography of secondary works and primary sources.

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ROBERT H. DAVIES *et al. The Kingdom of Swaziland: A Profile*. London: Zed; distributed by Biblio Distribution Center, Totowa, N.J. 1985. Pp. x, 82. Cloth $21.75, paper $7.50.

This is a clear, succinct introduction to the political economy of Swaziland. The book is comprehensive in scope, covering Swaziland under colonial rule, the process of decolonization, the structure of the economy, class formation and class struggle, factional conflict since the death of King Sobhuza and Swaziland's role in Southern Africa's regional struggle. Although the entire book is recommended to anyone interested in the political economy of Swaziland, the most valuable contributions are the analyses of the growing economic dependency on South Africa and of the transformation of the traditional Swazi aristocracy into a "royalist comprador bourgeoisie."

Robert H. Davies, Dan O'Meara, and Sipho Dlamini demonstrate that South African monopoly capital has come to dominate all sectors of the capitalist economy. Swaziland's economy offers limited opportunities for domestic capital accumulation. Furthermore, the country's geographical position, its dependence on essential imports from South Africa, and the prominence of receipts from the Southern African Customs Union as a source of public revenue have enabled South Africa to compel Swaziland to cooperate in the strategy of "regional deconcentration." But the authors move beyond presenting a classic case of dependency. The Swazi rulers are not merely hostages of South Africa, nor has the penetration of foreign capital destroyed their base of political power. On the contrary, Swazi royalists have used their control of the state machinery and the country's mineral assets to transform themselves from a traditional aristocracy whose material base rested solely on precapitalist forms of exploitation into a comprador bourgeoisie with a material base in the capitalist economy. In turn, the royalists have used the revenues thus gained to strengthen their political power. They have purchased additional land for the Swazi nation and provided financial support to key institutions such as the National Defence Force, the University of Swaziland, and various cultural groups. The result has been a "tight economic alliance" between the royalist comprador bourgeoisie and foreign capital.

In analyzing the process by which the traditionalist aristocracy has used its influence to gain a foothold in the capitalist economy, Davies, O'Meara, and Dlamini have made significant contributions to the literature on Swaziland, the study of political economy, and the nature of classes in Africa. Most previous works have emphasized traditional values and the charisma of the monarch. Although Swazi political culture and the leadership qualities of the

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monarch play an important role in shaping contemporary Swazi politics, one cannot fully understand the process by which the royalists have achieved and maintained control over the postcolonial state without closely examining the nature of the Swazi rulers' material base of power. Although the authors do not refer explicitly to the literature on class analysis in Africa, their data strongly support the contention that control of the state machinery, rather than of the primary means of production, constitutes the essential attribute of ruling classes in postcolonial Africa. Furthermore, even under conditions of extreme economic dependency, the Swazi ruling class has been able to engage in domestic capital accumulation sufficient to provide itself with a secure base in the capitalist economy. Whether the mechanisms of capital accumulation that the royalists have devised will give them the capacity to challenge the dominance of foreign capital remains to be seen.

The authors' description of the factional struggles following the death of King Sobhuza will be of particular interest to observers of contemporary Swazi politics. By confining themselves to a detailed account of the palace intrigues, however, Davies, O'Meara, and Dlamini fail to deal with important broader issues. First, the authors do not investigate the impact of the power struggle on the political legitimacy of the royalist regime. It would be of particular interest to determine whether factional conflict has affected the level of political support for the regime among the masses, especially among those engaged in subsistence agriculture. Furthermore, although the authors note the dissatisfaction that the dismissal of the queen regent provoked among teachers, students, and other elements of the petty bourgeoisie, they fail to explain adequately the basis of this dissatisfaction, because of their limited focus on the personal ambitions and machinations of the royalists. It is to be hoped that the authors will take up these broader issues in the expanded study they promise. Nevertheless, even as a profile, this book is a fine introduction to the political economy of a little-known but fascinating African country.

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ASIA AND THE EAST

FRANCESCA BRAY. *Science and Civilisation in China.* Volume 6, *Biology and Biological Technology*; part 2, *Agriculture*. Edited by JOSEPH NEEDHAM. New York: Cambridge University Press. 1984. Pp. xxvii, 724. $99.50.

TSIEN TSUEN-HSUIN. *Science and Civilisation in China.* Volume 5, *Chemistry and Chemical Technology*; part 1, *Paper and Printing*. Edited by JOSEPH NEEDHAM. New York: Cambridge University Press. 1985. Pp. xxv, 485. $89.50.

Stressing the uniqueness of Western civilization, Max Weber once wrote: "There was printing in China. But a printed literature, designed *only* for print and possible through it, and, above all, the press and periodicals have appeared only in the Occident." The question that Weber posed was, why had capitalism developed only in the West to facilitate the rapid advance of science and technology, much of it borrowed from China? Only the West, of course, had been able to build on the scientific advances that had originated in China. Joseph Needham asked a different question than Weber: Why did science and technology progress more rapidly in China than elsewhere but then lag behind the West?

The two volumes under review try to answer Needham's question. How well do they succeed? In terms of rich detail and the presentation of new knowledge of production methods and types of products in China, these works maintain the high scholarly standards already set in earlier volumes that date from 1970, when Needham's great undertaking first began. Both are beautifully illustrated, contain much useful information, and provide accurately translated commentary from original Chinese sources. They will serve as the standard references on paper, printing, and agriculture for years to come.

But I do not find satisfactory answers in either study to Needham's question. Francesca Bray's study makes a valiant effort to meet that challenge, but Tsien Tsuen-Hsuin's work does not.

Tsien presents an encyclopedic account of the origins of printing and its spread throughout China. He describes the art of paper making in great detail as well as the different forms of paper and their uses. In this way Tsien is able to show the assorted uses of paper in such forms as cups, fans, umbrellas, flags, lanterns, kites, and toys—products developed before the end of the sixth century A.D., nearly a thousand years earlier than in Europe. He does not ignore printing technology, the aesthetic aspects of printing, or the spread of paper and printing to the West and elsewhere. He also presents a useful discussion of the contribution of paper and printing to both the Chinese and Western civilizations.

Tsien poses a most interesting question that might have enabled him to organize his findings to answer Needham's question. He estimates that the cost of printing a single volume (*chüan*) of 5,000 to 10,000 characters in the T'ang period (618–906 A.D.) was ten times cheaper than the price of hand copying. But that cost ratio remained unchanged as late as the sixteenth century. Why? Tsien explores neither why printing costs did not fall further nor why a

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mass readership eager for cheap printed materials never evolved—the issue that Weber had raised about the difference between China and the West.

Tsien drops hints to answer Needham's question. He reminds us that books were reserved for a small elite whose tastes were concentrated on exquisite calligraphy and ornate uses for paper. He emphasizes that printing in the West was geared to making a profit but that in China books were supposed to contain knowledge, chiefly for moral development. But did mass consumption arise for some of the paper products that Tsien describes? If not, was that the reason technological change did not occur to reduce printing costs? Or did technological difficulties of paper making and printing prevent their supply from increasing at declining unit costs?

When we turn to agriculture, Bray must be congratulated for presenting detailed, accurate accounts of key farming handbooks, field systems, tools and their uses, and crop systems. At last we have the names of all important crops written in Chinese, a most useful reference tool. Planting calendars are well explicated. Farm tools are described with clarity. The work is a gold mine of information, but what of Needham's question?

Following P. Leser, Bray contends that China contributed to the eighteenth-century agricultural revolution in Holland and England. Dutch adaptation of the efficient Chinese turn-plow, with a curved iron mould-board from Indonesia (formerly used in southeast China) allegedly helped mechanize Dutch and, later, English farming. But, assuming that Bray and Leser are right, why did no agricultural revolution take place in China?

China did experience two great agricultural advances, according to Bray. The first came during the Han and after with large estates, greater irrigation, and improved methods of cultivating the arid soils throughout the Yellow River Basin. The second originated in the Sung period with the spread of double cropping. This second spurt, however, relied on small-scale farming and the intensive use of labor, all tied closely to commerce and handicraft, located largely in the countryside and organized by households in a labor-intensive manner. By the fourteenth century Chinese agriculture had become locked into a mode of production that would merely expand and be replicated over time without ever being transformed.

Are these views correct? Aside from the questionable importance of the modified plow in England, Bray ignores the cropping improvements based on new legumes that made for higher grain yields and increased fodder for livestock. Bray also ignores the role of changing factor prices, which made it increasingly profitable for English farms to enclose, specialize in the new crop rotations, and market a greater surplus of foodstuffs and animal products to the urban sector.

As for China, Bray downplays the importance of winter wheat and barley for increasing the output per sown acre in the three harvest per two year farming system that developed after Sui times. Finally, she does not analyze how improved farming technology, the role of the market, and Chinese customs and institutions facilitated so efficient an allocation of resources as to prevent any change in relative prices that might have induced suppliers to introduce new technology.

Why did China's biological technology promote a labor-intensive farming technology that supported enormous population growth? Mark Elvin has partially answered the question by referring to the great efficiency of markets. As yet, no scholar has been able to connect markets, the organization of farming, and the agricultural technology so elegantly described by Bray. But Bray's volume brings that challenge within reach, and for that we owe her a great debt.

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RICHARD L. DAVIS. *Court and Family in Sung China, 960–1279: Bureaucratic Success and Kinship Fortunes for the Shih of Ming-chou.* Durham: Duke University Press. 1986. Pp. xvi, 353. $37.50.

This book tells a fascinating story. The Shih family of Ming-chou on China's southeastern coast emerged from obscurity in the late eleventh century and rose to dizzying heights in the twelfth and thirteenth, only to sink back to relative obscurity under the Mongols in the fourteenth. Producing no fewer than three chief councilors and over two hundred forty civil officials during the Southern Sung, the Shih's success as a bureaucratic family was without parallel in Sung history. Richard L. Davis's chronicle of the fortunes of this great family is vivid and absorbing. After a brief description of Sung methods of bureaucratic recruitment and of the Southern Sung political context, he assembles fragmentary evidence to paint an almost classic picture of upward social mobility, from a sheriff's clerk's impoverished young widow struggling to feed and educate her only son to her grandson who, having passed the civil service examinations, rose to the position of assistant councilor during the 1140s. The heart of the book, however, concerns the achievements of subsequent Shihs, particularly Shih Hao, who served as chief councilor in 1163 and 1178, Shih Mi-yuan, whose twenty-five-year tenure in that post (1208–33) was the longest in the Sung, and Shih Shih-chung, who was chief councilor from

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1238 to 1244. Finally, we are treated to the vista of the Shihs in decline. Swollen in numbers, arrogant, and wed to the fortunes of the Sung imperium, the Shihs were deprived of power and prominence, and in at least one case brought to destitution and servitude, by the Mongol conquest.

As family history and as political history, for which Davis has a flair, this book is a valuable contribution to Sung scholarship. Questions arise, however, in his interpretation of the social significance of the Shihs. Reacting against a view of Southern Sung and later periods developed by Robert Hartwell and others in which local, tightly knit elites dominated society and perpetuated themselves largely through landholding, marriage ties, and access to office, Davis argues that Sung society was more fluid and open to upward mobility through the civil service examinations. Although these are reasonable concerns, his conclusions, unfortunately, overstep his evidence.

For example, Davis argues that the initial rise of the Shih through academic success provides a counterexample to Hartwell's claim that such success invariably followed intermarriage into the local elite, yet the lack of any information about the wife of the clerk's son Shih Chao leaves open the possibility that such intermarriage did in fact occur. Once established, moreover, the Shihs demonstrated not social mobility but the staying powers of privilege; only twenty-six of their two hundred forty officials passed the examinations, and even they may have benefited from less-competitive avoidance examinations, special entry into the Imperial University, and other privileges available to the kin of high officials.

Finally, we must ask how representative were the Shihs? The answer, I believe, is not very, for no other Southern Sung family enjoyed a comparable combination of longevity and power. In explaining their sudden fall at the end of the Sung, Davis speaks of "a false sense of security" that "effectively discouraged the saving of existing wealth for an uncertain future" (p. 184). This is persuasive for the Shihs, the long-term recipients of imperial largesse, but it hardly describes most of the elite literati, who were by no means assured of wealth and access to office and who often undertook a variety of strategies to insure the survival of the kin group. In this sense the Shihs appear as a throwback to the great families of T'ang, who exhibited a similar sense of self-assurance. Politically important they unquestionably were; socially representative they were not.

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ROBERT A. SCALAPINO and GEORGE T. YU. *Modern China and Its Revolutionary Process: Recurrent Challenges to the Traditional Order, 1850–1920*. Berkeley and Los Angeles: University of California Press, under the auspices of the Center for Chinese Studies, Institute of East Asian Studies, University of California, Berkeley. 1985. Pp. xiii, 814. $65.00.

Over twenty-five years ago Robert A. Scalapino and George T. Yu began their research for this study of "China's revolutionary process in its most extensive dimensions." The fruit of this ambitious undertaking is to be divided into three parts: the first part, the volume under review here, treats of the period from the Taiping rebellion to 1920; the second part will be a volume on the years 1921–49; and the third part will cover the period since the establishment of the People's Republic. When the work is completed—judging from this large, long, and highly detailed initial volume—it will be the most comprehensive portrayal of the men, ideas, and events that contributed to a century of revolutionary turmoil in modern China.

The topics covered by this first volume are unexceptional in themselves. The introductory discussion of the Taiping rebellion is followed by chapters on the reformist and revolutionary movements prior to the revolution of 1911. These include detailed descriptions and assessments of the T'ung-chih restoration, K'ang Yu-wei and Liang Ch'i-ch'ao, Sun Yat-sen, and the anarchists. Following these are chapters on the revolution that overthrew the dynasty, the political breakdown that followed the revolution, the development of the liberal and socialist movements prior to and during the May Fourth movement, and the early years of bolshevism. The volume concludes with a chapter on the formative years of Chiang Kai-shek and Mao Tse-tung, which includes a fascinating comparison of these two future national leaders.

The book is not just a survey, however, but a remarkably textured and critical examination of the above topics based on an impressive, indeed mind-boggling, amount of research. The authors, together with a sizable team of research assistants, have seemingly covered all of the relevant secondary and monographic literature in Chinese, Japanese, and English, including the unpublished proceedings of several conferences that have been held in China in recent years. Even more impressive, however, is that the bulk of the exposition is based not on this secondary literature but on research in a vast corpus of primary sources. The chapter on anarchism, for example, draws heavily on essays in the contemporary journals *Hsin Shih-chi* and *Min-pao*, and the analysis of Liang Ch'i-ch'ao's ideas is drawn not from the monographs of Chang Hao or Philip Huang but from Liang's original essays in *Ch'ing-i Pao* and *Hsin-min Ts'ung-pao*. As a source on Sun Yat-sen's early revolutionary activities, the authors

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shun the English translation of Miyazaki Torazo's *Sanjusannen no yume* (My Thirty-Three Years' Dream) by Eto Shinkichi and Marius Jansen in favor of the Japanese original edition of 1902. They have also combed through such sources as the *North China Herald*, the *Tung-fang Tsa-chih*, and the U.S. State Department records for historical data that have not previously been brought to light. In accomplishing this research, the authors have scoured through archives and libraries in Peking, Canton, Taipei, Tokyo, Paris, and Moscow, not to mention those in the United States.

This is, in several respects, a remarkable and highly successful history of the revolutionary process in modern China. It is richly detailed and is filled with fresh and significant information about the principal actors and thinkers that contributed to the revolutionary movement. It also contains well-balanced, theoretically informed assessments of events and the revolutionary dynamic. Its explanation, for example, of the differences in the responses of China and Japan to the commercial and industrial challenge of the West in the nineteenth century displays a profound knowledge of the history of the two countries. And, in their assessment of the 1911 revolution, the authors carefully examine—only to dismiss—the interpretations of the Marxists and other scholars such as Theda Skocpol, before offering their own explanation. Frequently, therefore, the work is both informative and stimulating.

Despite its many virtues, the work is to some extent disappointing, because it does not in any fundamental way alter or enlarge our existing understanding of the revolutionary process in China. If any theme courses through the entire work, it is the contrapuntal relationship between Chinese traditionalism and Western-inspired modernism—with the traditionalistic element seen ultimately to predominate. The concluding chapter tells us that China's revolutionary process was formed by the intermingling of five critical factors: human, institutional, cultural, spatial, and temporal. Although the discussion of these factors is invariably sound, it is also somewhat stale. One looks in vain for new and exciting interpretative insights into the revolutionary process as a whole. This is because the study is basically political and intellectual history of the same type that the past generation of students of modern China has been writing. It does not, that is, pose strikingly new questions, such as might have been suggested if the authors had devoted greater attention to the social and economic aspects of the period.

Finally, one wonders who are the intended readers of this eight hundred-page volume? The book is not a textbook; it is far too detailed for most university courses. Nor is it a monographic contribution, for, although much of the research is original, more detailed studies than this exist on most of the topics discussed. For serious students of the Chinese revolution, however, this will be a highly useful reference work and source of lectures. For graduate students, it will provide an excellent means of reviewing for the general examinations.

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JÜRGEN DOMES. *Peng Te-huai: The Man and the Image.* Stanford: Stanford University Press. 1985. Pp. xi, 164. $25.00.

This is an odd yet valuable study. Let me start with odd. The story of Peng Dehuai (Te-huai in the Wades-Giles spelling Domes uses) is one that begs to be told. Peng was the straight-talking peasant child with little formal education who rose to be a commander in the Kuomintang (KMT) and then in 1927, when the forces of the Communist party of China (CCP) were at low ebb, deserted to the Communists where he quickly became one of Mao Tse-tung's principal generals. After liberation Peng commanded Chinese forces in Korea and from 1954 to 1959 served as the minister of National Defense of the People's Republic of China. Seeking to reverse what he saw as the disastrous policies of the Great Leap Forward, Peng attempted and failed to face down Mao Tse-tung in July 1959. For this he was dismissed from his posts and then during the Cultural Revolution reviled as a villain, beaten, imprisoned, tortured, and otherwise hastened to an early demise. Since 1976 he has been posthumously rehabilitated as a hero of the Chinese people.

Clearly, this story could be one of the true epics of modern Chinese history—full of adventure and intrigue and brimming with insights into the nature of Chinese politics. But Jürgen Domes has oddly decided to stick to the dry facts of the case and write the tale of Peng in one hundred sixty pages including bibliography and footnotes. Further, since the author precedes each chapter on Peng with a well-done (but probably unnecessary for the specialists who are going to be reading this book) general history of the period about to be explored, the total text on Peng is well under a hundred, sometimes repetitious, pages, written in an extremely tedious manner.

The book is also odd because Domes never really explains the enigma of Peng. Domes writes that Peng had little interest in politics before the 1950s, was poorly read, and was, to boot, a mediocre strategist. To be sure, he was also known as a great organizer with a common touch and for his overriding sense of loyalty to the party. Yet this hardly explains Peng's rise from foot soldier to the upper levels of command within the KMT and then to the

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highest reaches of the Communist hierarchy. There is little here that shows Peng's dazzling ability to awe almost every foreigner he came into contact with from Edgar Snow to dour and sceptical Soviet bureaucrats. Nor does this description of Peng explain the independence with which Peng somehow managed to operate. In his earlier career he switched easily and without repercussions from the side of the northern warlords to the KMT and then to the Communists. In his later career he seemed to have no trouble ignoring at times the dictates of Mao Tse-tung and other top leaders with whom he disagreed, and yet, until his final confrontation, he somehow still retained their respect and trust.

Now for the valuable part. The legend of Peng Dehuai, both as hero and as villain, is one that has been repeated for at least twenty-five years both within China and among those who study the country. This book is the first to set forth the facts of a career that had previously been obscured by the stories and controversy surrounding it. Moreover, Domes also has some exceedingly interesting things to say about the nature of factional politics in China. He convincingly shows that Peng's fate was in the end so severe because, as the result of having repeatedly changed commands, he was one of the few major military leaders with little substantial personal following and was therefore an easy sacrifice for military leaders to make to the leaders of the Cultural Revolution as part of a deal that allowed the military to continue to share power without too much interference. And just as the leaders selected Peng as a convenient target of the Cultural Revolution so they also later selected him as a hero, whose positive image among the masses could help win support for some of those erstwhile friends who had abandoned him in 1959. Amazingly, Domes makes these points in a few pages toward the end of the book. Although these themes certainly could have been more fully developed and documented, the points come across. And, in the process, this study inadvertently demonstrates the waste of time and space expended by most of us who feel compelled to decorate our ideas with often irrelevant locution.

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WILLIAM W. KELLY. *Deference and Defiance in Nineteenth-Century Japan*. Princeton: Princeton University Press. 1985. Pp. xvi, 322. $31.50.

Combining the field methods of the ethnologist with the archival research of the historian, William W. Kelly has produced a stimulating study of four protest movements in Shonai domain of northern Japan. Although his main purpose is to probe the character of and relations between these popular protests and to determine their broader significance, he offers much more. His description of the human and political geography of Shonai and his rich analysis of developments there during four decades divided by the Meiji Restoration is an engaging slice of local history, which is only a rewarding by-product of this book.

The heart of the book—seven of eleven chapters—analyzes the causes, participants, demands, and justifications of the protest movements. Each had different causes and outcomes. The first, in 1840–41, was a collective protest consisting of petitions, pilgrimages, and mass assemblies of peasants, village officials, and merchants to block a shogunal order transferring the daimyo of Shonai. The order was canceled and the status quo preserved. In the second disturbance (1844) the domain was the target of sake brewers and peasants, who, anticipating new levies, prevented the authorities from taking over shogunal lands within Shonai.

The two protest movements following the restoration were unusual in several respects. In 1869–71, in the wake of the defeat of Shonai domain, which had supported the Tokugawa cause, protests arose over a series of new taxes and administrative reforms following the partition of the domain. Old retainers, supported by petition-bearing villagers and financed by a wealthy local merchant-landowner, lobbied in Tokyo and won concessions. The final protest, which lasted longer (from 1874 to 1876) and was more complicated, stemmed from the rural insecurities resulting from the shift to agrarian capitalism. Sustained complaints and law suits were directed against the traditional village elite, now in the service of prefectural authority, because of harmful commercial activities and tax arrangements (one charge being that the national land tax reform of 1873 calling for cash payments was being ignored). Arrests were made and litigation in Tokyo continued for years. Some restitution was awarded, but Kelly concludes that "the central government was the ultimate victor" because prefectural officials were warned against disobeying national laws (p. 259).

Kelly's study is a valuable contribution to the growing literature on peasant protests and uprisings in the formative years of modern Japanese history. The current popularity enjoyed by this approach derives from the belief that the neglect of conflict in Japanese society distorts its history. The author's case studies advance this line of inquiry, but his judgments are balanced and cautious. He does not find the protests to have been class based, revolutionary, or even very violent. Instead, he stresses the diversity in the class composition of participants, the variety of their claims and demands, and dissimilarities in their targets and final effects. They shared certain features, but, in the complex interactions of

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peasants, merchants, and prefectural officials, the Shonai protests were examples (undramatic but perhaps typical of nineteenth-century rural conflict) of Japan's struggle to transform itself into a centralized nation with a capitalist economy.

ROGER F. HACKETT
University of Michigan

SHARON MINICHIELLO. *Retreat from Reform: Patterns of Political Behavior in Interwar Japan.* Honolulu: University of Hawaii Press. 1984. Pp. xi, 172. $18.00.

Sharon Minichiello's book reviews the thought and career of Japanese party politician Nagai Ryūtarō as a representative of the group Itō Takashi has called "reformist" in modern Japanese politics. In Minichiello's view Nagai's transition from liberal democrat to spokesman for military totalitarianism should be seen less as an ideological apostasy (*tenkō*) than as a typical interwar pattern of political behavior by "reformists" (*kakushin ha*). She focuses primarily on "why those Japanese seemingly so attracted to Western liberal ideals and systems in the 1910s and 1920s began to reject that same liberalism in the 1930s and opt for more totalitarian models, both in the national and international spheres" (p. 21). Minichiello also illustrates through Nagai's career the theses of Robert Bellah and Maruyama Masao on how difficult it has been for Japanese to adhere to universal values that transcend or challenge the particularities of Japan's traditional social and political ethics.

Well-educated in Western thought at both Waseda and Oxford, Nagai (1881–1944) flirted briefly with socialism before becoming an eloquent advocate of liberalism and democracy during the 1910s and 1920s. His election to the House of Representatives in 1920 reflected both the political attractiveness of these ideas and the new political value of oratory in a burgeoning mass society. Minichiello persuasively situates Nagai among the "reformists": young men who sought in the wake of World War I to overthrow *hanbatsu* government in the name of popular welfare. They also hoped to assume national leadership themselves in a restructured system that would strengthen Japan internally and permit Japan to defend its interests among the powers more forcefully. Nagai corresponds to the "progressive-reformist" subtype, those who favored a range of ideas spanning communism, socialism, and liberalism. Itō (and Minichiello) hold that, by the mid-1930s, this subtype had disappeared from public life or had merged with the "restoration-reformists" (right wing). Minichiello adroitly shows how Nagai traversed this course and how Japan's international crises and ambitions gradually stimulated his interest in totalitarian politics and military expansion.

Minichiello's careful study reminds us that liberalism in Japan, as elsewhere, was not necessarily synonymous with pacifism or antistatism. The road from "progressive" to enthusiastic supporter of the new order in East Asia and new political order in Japan was a surprisingly direct one for Nagai and other "reformist" fellow travelers. Perhaps one reason for their ultimate hostility toward the West and reaffirmation of "particularistic" Japanese notions of ethics and morality lay in justifiable doubts that even Westerners would adhere faithfully to liberalism or the principles of international and domestic justice articulated in the peace treaties and arms limitation agreements of the interwar era. The widespread failure of commitment to "universal" values and principles on both sides of the Pacific thus might well be studied with a seasoned skepticism about how universal they really were.

GORDON M. BERGER
University of Southern California

DIETMAR ROTHERMUND. *Indiens wirtschaftliche Entwicklung: Von der Kolonial Herrschaft bis zur Gegenwart.* (UTB für Wissenschaft, Uni-Taschenbücher, number 1378.) Munich: Ferdinand Schöningh. 1985. Pp. 228. DM 29.80.

Dietmar Rothermund presents a useful primer about Indian economic history, which he calls "a spanned drama" over various periods and phases. Rothermund deals with early peasant cultures, military-feudalistic regimes, Mogul empires, and British colonialism in terms of an alien parasitic capitalistic system intertwined with the Indian agrarian economy. He leads from the regime of the colonial lords to the postcolonial period and presents the Indian dilemma of how to approach economic development: through speedy industrialization or through agricultural growth, which was instituted after the first three five-year plans failed. Rothermund discusses *in passim* some of the consequences of the breakthrough in agricultural technology (the Green Revolution), and he also deals with the current problems of urbanization, migration, and regional disparities. The book ends with the events of the early 1980s.

Those who do not know India at all and suppose that the subcontinent consists of the splendor of the Taj Mahal and Westernized middle-class Indians who call their fathers Daddyji, or who imagine India in extremes from the boutiques of the Grand Hotel Oberoi to reptile-infested swamps, poor arid lands, and backward tribal areas inhabited by impoverished people without any sense of history can learn a lot from Rothermund's book. Specialists in South Asian history or economics, however, know that a lot has been written on India based on "well-proved"

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suppositions; if they seek a new hypothesis or new set of data, they will find this book partly repetitious.

The study expresses the author's obvious sympathy with the cause of Indian independence and the struggle of its preindependence politicians for their homeland and their values. His compassion, however, does not obscure the power of his judgments and, in fact, enables him to demonstrate the historical evolution of the British-Indian relationship with some of its complexity.

The book is directed to the general public interested in the economic history of India, and it can be valuable as a first reading for students whose aim is to specialize in some area of the Indian subcontinent.

PAUL JONAS
University of New Mexico

ANAND A. YANG, editor. *Crime and Criminality in British India.* (Association for Asian Studies Monograph, number 42.) Tucson: University of Arizona Press. 1985. Pp. xi, 192. $17.95.

This book is the forty-second in the generally splendid series of monographs published by the Association for Asian Studies. Given the enduring fascination of crime and punishment, it is also one of the most intriguing volumes in the series. The introduction by Anand A. Yang, who edited the volume, sets the appropriate tone in discussing the history of scholarly trends and schools of thought in the study of crime and criminality. John McLane's excellent essay concentrates on the situation in Bengal in the first years after the permanent settlement. Not surprisingly, the *dacoits* were drawn from the lower orders of society—village watchmen, laborers, cultivators, servants, fishermen, cowherds, beggars, and so on. McLane contends that the local landholders probably could have eliminated the *dacoits*, but many of them profited from sponsoring the bandits or helping them dispose of stolen goods. Of course, a major catalyst for banditry was the permanent settlement of 1793, which, as McLane points out, separated authority and power from property.

Bruce Robinson writes on the bandits of western India and comments on the social role of the armed gangs and the bandit chief as the righter of wrongs and public hero. Chapter 3, by David Arnold, moves the area under investigation to Madras in the years 1858–1947. He emphasizes the role of crime as protest and notes that the British reaction was not one of redress for victims of individual criminal acts but rather of concern about group acts as symptoms of incipient revolt.

Chapter 4, by Edith Brandstadter, deals with the curious tale of the Konds and their propensity for human sacrifice. After the Goomur War of 1837–38 the British authorities used various socioeconomic strategies to "civilize" the Kond. They supported the establishment of social, political, and economic networks that seemed to be leading to the increased "hinduization" of the tribe. The British even managed to gain the acceptance of buffalo meat in place of human flesh for Kond ceremonies.

Yang next discusses the Criminal Tribes' Act of 1871 and the British propensity for categorizing groups of people into generalized classes such as warrior races, criminal tribes, and dangerous castes. The policy probably simplified administration but was hardly calculated to advance the cause of individual human rights. In that sense it was in stark contrast to developments in Britain itself.

Stewart Gordon uses the Bhils as a case study of the rehabilitation of a criminal tribe, and Sandria Freitag concludes the volume with an essay on collective crime and authority in northern India. He cogently discusses the development of a philosophy of effective crime control originally inspired by the establishment of revenues from land as the chief source of income for the state and the consequent commitment to protect settled agrarian communities.

Monographs, by their very nature, are usually for the eyes of the specialist. This book may well be an exception, for it belongs on the bookshelf of any scholar concerned with the history of British rule on the subcontinent.

ROBERT A. HUTTENBACK
University of California, Santa Barbara

CHAR MILLER, editor. *Missions and Missionaries in the Pacific.* (Symposium Series, number 14.) New York: Edwin Mellen. 1985. Pp. 125. $19.95.

Reviewing this volume reminds me of the task of commenting at an all-too-typical session at a professional meeting: it consists of three papers of widely disparate focus and quality, related only in being small pieces of the same general subject, and leaves the commentator vainly struggling to develop a common theme. The first of the three essays, James Boutilier's "We Fear Not the Ultimate Triumph: Factors Effecting the Conversion Phase of Nineteenth-Century Missionary Enterprises," lists some ten variables in a complex interaction: the intentions of missionaries, the intermediary role of beachcombers, the degree of hierarchy in indigenous religion, the readiness of the political elite to seize on the religion to bolster its power, and so on. He then illustrates these one by one with examples drawn mainly from secondary works and concludes by repeating the list. In lieu of transitions, his thirty-nine-page piece contains no less than eighteen sec-

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tions separately subtitled. And, somewhat disconcerting in view of the subject, the word "deities" is frequently and consistently misspelled. The writing in this essay, in my judgment, is not up to publishable standard.

The essay by the editor, Char Miller, entitled "Domesticity Abroad: Work and Family in the Sandwich Island Mission, 1820–1840," examines the contradictory and traumatic interrelationship between hard realities and the goal of providing examples of functioning Christian families. Like other Europeans who lived in the tropics, missionaries employed native servants, sent their children "home" to school, or left themselves—but they thought, worried, and wrote about the problem at length. Based mainly on diaries and other primary sources, Miller's tightly argued, artistically cast, and well-written essay gets close to the lives of missionary women.

Last is Charles Forman's "Playing Catch-Up Ball: The History of Financial Dependence in Pacific Island Churches," which begins with a singularly bland statement—"One of the problems of the so-called developing nations, as they enter the world economic system, is that they seem trapped in a position of dependence on the developed nations" (p. 91)—on which it somewhat improves. Early missions, it seems, living off the modest contributions of the island villagers they served, were usually self-supporting. But, like jet airplanes, such symbols of modernity as theological training, outreach programs, and conferences seemed essential for respectability, and these activities far outstripped resources. Thus, Pacific missions, like those in Asia or Africa, remained dependent on Western sponsors.

Symposium volumes rarely make good books. The rationale for them, I suppose, is that several writers can achieve more range and depth than one. It seldom works that way. This volume is poorly produced, overpriced, and badly proofread. I hate to be churlish, but Miller's interesting essay would have been better placed in *Signs*.

JOHN W. CELL
Duke University

NORMAN G. OWEN. *Prosperity without Progress: Manila Hemp and Material Life in the Colonial Philippines.* Berkeley and Los Angeles: University of California Press. 1984. Pp. xxii, 311. $30.00.

Norman G. Owen has written an interesting economic history of the Bikol (Kabikolan) region of the Philippines during the nineteenth century. Situated in the southeastern part of the northern island of Luzon, the area was portrayed as a backwater by contemporary sources at the beginning of the period under study (p. 39). Reality changed dramatically after 1820, when the economy of Bikol became integrated into the world economy as a producer of abaca (manila hemp). After World War I, the market declined, and so did the region; few effects of the boom of the previous century remained.

Owen draws on the concept of a capitalist world economy as developed by Fernand Braudel and Immanuel Wallerstein, but the author acknowledges that the model is of use in understanding events in Bikol only in the nineteenth century (p. 32). Although the area was administered by the Spanish colonial authorities in Manila, it was economically and geographically isolated from Manila, the rest of the Philippines, and the world economy. The population was relatively homogeneous; the economy was self-sufficient in rice and most other items; and the colonial superstructure, apart from the tobacco monopoly (1782–1882), had relatively little impact. Marauding Muslim pirates from the southern Philippines (Moros) were the most significant contact that the region had with the outside.

Abaca changed all that. The fiber came into great demand for ships' rigging (by the Civil War only abaca was used on American vessels for running rigging [p. 47]) and, later, binder's twine. In 1818 fourteen tons of abaca were shipped to Salem, Massachusetts. With fluctuations, exports from Bikol increased until the period 1910–15, when they averaged over ninety thousand tons per year. Thereafter, the industry declined in the region, both absolutely and relative to other areas of abaca production in the Philippines.

Tracing the link between Bikol and the global economy, Owen focuses on the agency houses in Manila and examines the impact of the initial surge in demand for abaca and the later fluctuations in the market. The changes were largely a matter of degree. Abaca was complementary to rice cultivation; therefore, the pattern of ownership in the lowland area was not disrupted. The labor system in abaca production rested on a combination of wage and share payment. In the author's view, however, the workers remained essentially peasants, keeping one foot in the subsistence sector (p. 114).

Nevertheless, the region lost its self-sufficiency in rice production and in general became more economically attached to the national economy. Much of the abaca-growing area came under the ownership of outsiders, and Bikolanos began to migrate in response to differential economic opportunities. The region, however, was unable to transcend its dependence on abaca. When the market declined, so did the region's prosperity.

Throughout the book Owen points to limitations in available information and the need to draw inferences. Many of his comments are qualified, and he does not provide a general discussion of his data. Another problem is that the author does not investigate the reasons for the inability of the Bikol

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region to survive the decline of the abaca market. In particular, Owen includes relatively little discussion of the generation and use of the economic surplus. Toward the end of the book, Owen does refer to "a persistent drain of resources from Kabikolan toward Manila—of money, produce, and eventually human talent" (p. 210), but the subject is not explored in detail. Nor is the importance of the region's class structure on economic events taken up.

These concerns notwithstanding, Owen's monograph is a welcome addition to economic histories of the Philippines and a significant contribution to our understanding of the economic development of that country.

CHARLES W. LINDSEY
Trinity College

PATRICIA GRIMSHAW *et al.*, editors. *Families in Colonial Australia*. Winchester, Mass.: Allen and Unwin. 1985. xv, 227. Cloth $27.50, paper $14.00.

This collection of sixteen papers, varying in length between five thousand and twelve thousand words, reflects the state of family history in Australia, although some of the best recent research is not represented. The quality is uneven. The ambit of some papers is so narrow and the research for others so slight as to suggest the rush to publish is the reason for their inclusion, but others make more substantial contributions to family history and, at best, to the national history. The authors include a librarian, an educational historian, and two demographers. The editors, Patricia Grimshaw *et al.*, defend the disparity as a decision by Australian scholars to avoid a "thermos flask" approach to family history.

Among those of narrow ambit are studies of some Irish families in Victoria between 1851 and 1891 and of Scots arriving in Victoria between 1852 and 1854 from whom a few families are traced to 1891; a statistical analysis of family structure in a small rural community in southern Queensland in 1870 and 1889; a discussion of domestic violence hinging on one horrendous divorce case in Victoria in 1870; an examination of 902 applications from convicts for permission to marry in 1838—many written on behalf of the applicant—in order to ascertain motive for choice of partner; and an impressionistic account of death drawn from a few Western Australian letters. The techniques of historical demography are brought to bear on family formation in Western Australia between 1842 and 1849, Castlemaine, Victoria, from 1861 to 1891, Bendigo, Victoria, 1881 to 1901, and the Horsham district in Victoria between 1862 and 1899.

In the investigation of Castlemaine the statistical data underpin a discussion of women's life experiences and domestic labor. In another study of Illawarra, a coastal region of New South Wales, church and school records are the basis of a comparison between a mining and a farming community. No comparisons have been made between these studies, so facts accumulate about such matters as the modal age of brides and the average number of children born to different age cohorts, and sometimes the national statistics are assembled to mark regional variation, but what it amounts to is not readily apparent. The editors claim that a broad documentation of everyday life will deepen understanding of the "context of power in past society" (p. xv), but individual pieces often are innocent of any interest in power, authority, and oppression.

An exception is Marilyn Lake's "Helpmeet, Slave, and Housewife: Women in Rural Families, 1870–1930," in which power relationships within the family are made central to the investigation in the context of often considerable poverty and the prevailing ideology regarding the housewife. Shirley Fisher in "The Family and the Sydney Economy in the Late Nineteenth Century" also examines power relations, more particularly the economic forces that impinge on family life, and avoids simplistic assumptions about the imposition of the bourgeois family model. Very different, but also of significance for the national history, is the application of the incomplete data methodology, which demographers developed for studying third-world populations, to census data from colonial Victoria (manuscript census returns have not survived). Ellen McEwen was left with the impossible task of trying to sum up contributions that do not sum.

HEATHER RADI
University of Sydney

UNITED STATES

LEWIS PERRY. *Intellectual Life in America: A History*. New York: Franklin Watts. 1984. Pp. xvii, 461. $20.00.

Ever since the 1960s, Lewis Perry asserts, the history of intellectuals cannot be regarded as "central to everyone else's" (p. xv). Nor, he notes, have historians reached any consensus about the nature of intellectual life. His themes emerge from these quandaries. His response is to focus on the "social functions of intellectual activity in different eras" (p. xvi) and their importance in the eyes of other Americans. Entangled with this thread is the changing view of the place of intellect, leading to the theme of how emotions became dominant in "common views of morality" (p. xvi). Looking at broad patterns more than at individual thinkers, he has

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written a survey and included helpful recommendations for further reading that are especially addressed to preprofessional students. The book ends with a portrait of our current disarray in which we seem to be "vaulting into the future" but may be aiming "to recover something lost" (p. 384). Stressing the prevailing pattern, unfortunately, works against detailed treatment of individuals and ideas, yet the charm of intellectual history derives from engagement with persons and their ideas. More than any other field, intellectual history is injured by textbook treatment because ideas lose their power when they are treated in descriptive, summarized terms. The result is to promote the making of "safe judgments" from which the jacket-flap promises to deliver us.

Perry includes Puritans, southern gentlemen, evangelicals, republican ideologists, Victorian defenders of the genteel tradition, and pragmatists but does not explain why he eliminates judges, nuclear scientists, architects, and modern historians from his broad category of intellectual activity. Focusing on the "prevailing," he has to condense to a mere two sentences the challenge to the commonplace found in the work of Nathaniel Hawthorne and Herman Melville. As he moves into the twentieth century, when a distinctive, self-conscious intelligentsia emerges, his narrative acquires more shape and cogency.

Coming down into the present, however, is always a bumpy ride for historians because they require hindsight to establish perspective. Seeing the end of the social role of the Puritan preacher, the republican gentleman-statesman, the genteel Victorian apostle of "culture," or the "man of letters," he does not convince me that Christopher Lasch's idea of "narcissism" is a convincing historical explanation. Historians will find here, however, helpful analysis of "culture" as a Victorian ideology, a needed reminder of the importance of American anthropologists in demolishing Victorian certainties, and a proper respect for the renaissance in social theory and literary criticism promoted by those who "repudiated Stalinism from close experience" (p. 407). I think they will be dissatisfied with the neglect of proslavery theorists (especially George Fitzhugh) or abolitionist thinking and disappointed that Jefferson's Statute for Religious Freedom, which has had an extraordinary second life since 1947, is cited only as "a tactic to protect the political hegemony of the large landholders" (p. 124).

The sociology of intellectuals fascinates modern intellectuals, but I agree with J. P. Nettl that the place to begin is with ideas, rather than with the social functions of intellectuals, even if one's interest lies in the literary, intellectual, or political movements that ideas and people have inspired. Because of his emphasis on function, Perry finds something "ephemeral" and "contradictory" about the Puritan clergy as an intellectual class because it was firmly located in "a scheme of power and subjection" (p. 90). Yes, but to speak of Puritans in the terms Randolph Bourne used to criticize the Deweyean pragmatists in wartime is anachronistic.

My problem with this book is that the two themes—the place of intellect in our views of human nature and the position of intellectuals in society—are awkward partners so that this effort to marry survey and analysis, intellectual history and the sociology of intellectuals, seems much more acceptable to the justice of the peace than to the bickering couple.

CUSHING STROUT
Cornell University

JOHN M. FINDLAY. *People of Chance: Gambling in American Society from Jamestown to Las Vegas*. New York: Oxford University Press. 1986. Pp. vi, 272. $19.95.

John M. Findlay's new book equates Americans' fascination with games of chance to the frontier experience. According to Findlay, a relatively fluid society endured in the far West into the twentieth century. "Successive Wests in the United States, populated by those engaged in resettling and pursuing fortune, provided the most productive milieu for the cultivation of novel and distinctly American betting practices," he writes. "To explain such characteristically American phenomena as quarter-horse races, riverboat gamblers, or Las Vegas casinos, it is essential to understand their western origins" (p. 5). In short, Las Vegas and attempts to copy its success—from lotteries in Missouri to casinos in New Jersey—represent the perfection of links extending back to Jamestown.

Both public and private authorities have taken advantage of people's expectations and willingness to gamble to acquire quick wealth. In colonial times many governmental units sponsored lotteries. The Continental Congress even tried to use a lottery to help finance the Revolutionary War. Lotteries gradually fell out of favor, because of corruption and irregularities. During the Jacksonian period reformers successfully supported legislation that made lotteries illegal on the grounds they corrupted people. That did not end gambling. Faro and keno remained popular in the West; Andrew Jackson was an ardent gambler who as president kept a string of race horses. The southwest frontier saw the rise of New Orleans as a gambling and entertainment center. Riverboat gambling flourished on inland waters during the steamboat era. Gambling played a prominent role in all the great western mineral rushes; after all, most miners were speculators. From time to time, vigilance committees hanged gamblers, ei-

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ther to punish dishonest ones or to emphasize the need for a more moral and settled society. Still, gambling continued to flourish into the twentieth century.

Of special importance was the rise of legal gambling in Las Vegas, Nevada. Until after World War II local boosters thought of the city more as a resort town than as a gambling center. Change came when Benjamin "Bugsy" Seigel constructed a large and successful multipurpose resort center in 1947. In short order entrepreneurs—many with ties to gangsters—built several resort gambling casinos on Fremont Street and along the strip. Although Hollywood celebrities and various gimmicks lured tourists, gambling was the biggest single attraction. A growing prosperity based on a business that was illegal in many parts of the country made Las Vegans uneasy about their national image. To compensate they took militant anticommunist stands and pointed with pride to nearby atom bomb tests. A public-relations problem, never successfully overcome, was that gambling towns by their nature draw an unsavory element.

Findlay's pioneering study is based on a wide variety of primary and secondary sources, ranging from federal racketeering hearings to Las Vegas promotional literature. He breaks new ground by structuring and analyzing gambling in a historical context. About half of his book covers the rise of Las Vegas, which leaves the book a little out of balance. Moreover, the sections on Las Vegas focus too much on boosters; the connection between organized crime and casino gambling is understated. Still, Findlay's interesting and provocative book stands as a major contribution to an important but all too often ignored aspect of American social history.

LAWRENCE H. LARSEN
University of Missouri, Kansas City

CARL J. EKBERG. *Colonial Ste. Genevieve: An Adventure on the Mississippi Frontier*. Gerald, Mo.: Patrice. 1985. Pp. xxi, 541. $28.95.

Carl J. Ekberg has written an informative and scholarly account of one of the initial European settlements on the west bank of the Mississippi River. A masterly command of sources, useful illustrations and appendixes, and a sharp, clear narrative combine to re-create a unique frontier community differing from settlements in Louisiana and the English colonies alike.

At first glance the history of Ste. Genevieve might be dismissed as the story of a small and unexceptional place, especially when compared to such centers as St. Louis. But Ekberg demonstrates that Ste. Genevieve was by no means a mere satellite of its notable neighbor upstream. On the contrary, although the two towns had much in common, such as a predominantly French population, black slavery, and dependence on the river for commerce and communications, Ste. Genevieve was set apart. Ste. Genevieve's economy was based on agriculture, especially the production of grain in the enclosed Big Field, and to a lesser extent on the exploitation of nearby lead and salt deposits. Unlike St. Louis, the town was never important in the fur trade. Instead, it was founded by agrarian families of French Canadian stock who crossed from the Kaskaskia side of the river to open new land for cultivation. Although it never became a large community, was often subjected to floods, and practiced antiquated agriculture, the settlement did become, for a time, the center of the most productive grain region of Spanish Louisiana.

To some degree, the survival of Ste. Genevieve between its founding, which Ekberg leaves no doubt occurred about 1750, and the American annexation after 1803 was a matter of adjustments to the requirements of nature, international trade and conflict, and French and Spanish colonial policies. But Ekberg's treatment of the pioneer families indicates the importance of the human factor. The determination of the iron men and women, the practical, adaptable folk, such as the Vallés and the Datchurats, who endured administrative changes, Indian hostility, clerical intrigue, and even a movement of the town to higher ground, sustained the little society. Guided by self-interest, they resembled the St. Louis Chouteaus in placing family and home before flags and governments. But, also like the St. Louis French, their eventual eclipse was assured by the arrival of the unbridled and ambitious Americans, perhaps personified by the enterprising lead miner Moses Austin. The great river that nurtured and occasionally threatened Ste. Genevieve for a half century could not hold back the far more decisive Yankee flood.

This is a good story well told. Divided into three broad sections dealing with origins, daily life, and organization, the book succeeds best in part 2, entitled "Fundamentals of Life." Particularly in his accounts of slavery, work, medical practice, and parenting, Ekberg vividly recaptures the experience of French life on the Mississippi. Scholars and general readers will profit from this excellent work.

DAVID RICE
Central Missouri State University

ALLAN KULIKOFF. *Tobacco and Slaves: The Development of Southern Cultures in the Chesapeake, 1680–1800*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture, Williamsburg, Va. 1986. Pp. xviii, 449. $30.00.

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Studies in the social history of the early Chesapeake continue to proliferate. Allan Kulikoff's book is the first major effort to synthesize this literature and present an overarching interpretation that links the economic, social, and political development of this region from the seventeenth to the early nineteenth centuries.

Kulikoff groups people into categories such as age, sex, race, and class. The history of class furnishes the book's unifying theme. Although Kulikoff assigns tobacco a formative role in the evolution of Chesapeake societies, his interest centers not on the economy itself but on the social relationships it generated, particularly in the wake of large-scale importation of slaves from Africa beginning in the 1680s.

After taking the reader through heavily detailed surveys of demographic and economic developments in the region, Kulikoff turns to an analysis of the evolution of white society in terms of class and then to black society. The author does not tell his story in simple terms of villains and victims but portrays his protagonists as sentient beings making choices, forging alliances, and conceding ground in one place in order to take ground in another.

The transformation of wealthy men in the older sections of tidewater Virginia and Maryland into a nearly hereditary ruling class occupies Kulikoff's attention for most of the book. He argues that propertied whites divided into two classes, gentry and yeomen, but that the gentry did not finally achieve political and cultural hegemony until the middle decades of the eighteenth century. Passage of tobacco inspection acts signify this triumph, but Kulikoff also notes that opposition was apparently defused by the succeeding wave of extraordinary economic growth. The gentry used its wealth to monopolize local political offices for its sons and to acquire the education and accouterments that authenticated its pretensions. Gentry hegemony survived intact into the 1820s, Kulikoff argues, and successfully breasted challenges posed by the political, religious, and economic upheavals of the revolutionary era.

Crucial to keeping the loyalty of the yeomen, the author asserts, was the patriarchal family structure inherited from England. Its values helped shore up the authority of adult white males over their families as well as over the slaves and thus promoted solidarity among all white men.

The scope of the author's vision and the extraordinary breadth of his research commands respect. He has built on his own and Lois Carr's pioneering use of the records of Prince George's County in Maryland and on the published and unpublished studies of scores of scholars in both colonies. (North Carolina is not included.) This very comprehensiveness poses problems because the findings to date are uneven, incomplete, and often incompatible. Kulikoff attempts to shoe-horn these data into a sequential model that also tries to take into account differing stages of development between tidewater and piedmont. His topical and chronological organization, moreover, forces the reader to wade through interminable thickets of numbers perpetually qualified by place and time, often without learning their relevance for pages on end. Had Kulikoff organized his material strictly in accordance with the key points of his argument, much of the existing text could have been jettisoned and repetition avoided, thus leaving ample room for a reasoned evaluation of pertinent evidence as well as space for a much-needed bibliography.

Given the ambitious nature of this work and the unevenness of the material, we must regard the book as exploratory and experimental. It will arouse useful debate on specific issues and force a reexamination of questions and goals for colonial historians. Kulikoff's book deserves to be read not only because it presents a useful survey of recent works in Chesapeake social history but also because it provides both a map and an agenda for the future.

GLORIA L. MAIN
University of Colorado

THOMAS J. CURRY. *The First Freedoms: Church and State in America to the Passage of the First Amendment.* New York: Oxford University Press. 1986. Pp. viii, 276. $24.95.

In an unpublished memorandum on the First Amendment clauses on religion, later appearing in his book *Judgments* (1972), Leonard W. Levy offered suggestions for a definitive treatment of the revolutionary generation's understanding of these clauses. Recognizing the paucity of direct information on intent in the published records of the First Congress, Levy directed future researchers to state documents, pamphlets, and the writings of participants but also indicated that the colonial experience was relevant as well. Thomas J. Curry, a doctoral student of Levy, has now followed up his mentor's earlier advice with this book.

Announcing that he will "elucidate the intentions underlying the First Amendment's religion clauses by framing the concepts . . . within the overall picture of colonial and revolutionary America" (p. viii), Curry traces the ideas of religious freedom and establishment of religion from the seventeenth-century Puritan Way through the eighteenth-century colonies and the revolutionary states to the adoption of the Bill of Rights. He particularly directs attention to cases of church-state conflict in which clashing values and motives surfaced in the heat of contention.

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Curry finds wide support in the states for freedom of worship by the time of the revolution. For "most" Americans the corollary to this was agreement that government should not support the churches by regulations or taxes. Some Americans disagreed with this view, principally in New England, but nine of the eleven states ratifying the First Amendment saw state support as a violation of liberty of conscience and the creation of an established religion.

The First Amendment was intended to reassure the states that the national government was powerless to interfere with their hard-won religious freedom. The congressional debates over the phrasing of the clauses on religion did not reflect differences between parties wishing "to give the federal government more or less power in religious matters. It represented rather a discussion about how to state the common agreement that the new government had no authority whatsoever in religious matters" (p. 215).

Curry's thesis is not entirely novel. It recalls the neutralist interpretation of the First Amendment's religious clauses, which maintains that these provisions were intended neither to advance nor retard religious practice in the states. But neutralism has seldom been argued with the grasp of complexities and the feeling for ambiguities and contradictions that it receives here. Nor has it been so carefully rooted in the historical setting as Curry succeeds in doing. This book can stand with Levy's work on the press and speech clauses of the First Amendment. Not only historians but also judges and lawyers should consult this book.

JOHN W. PRATT
State University of New York, Stony Brook

ELAINE FORMAN CRANE. *A Dependent People: Newport, Rhode Island, in the Revolutionary Era*. New York: Fordham University Press. 1985. Pp. x, 196.

The central objective of this study is to explain the rise and swift fall of one of colonial America's leading ports. To this end three interrelated topics—"the basis of Newport's economy, the effect of that economy on individuals and groups within the community, and the consequences of Great Britain's role" are explored (p. 4). Elaine Forman Crane concludes that: Newport was dependent on Atlantic and Caribbean trade; the harmony of its diverse population was maintained by mutual interests in commercial prosperity; and the disruption of trade by the imperial reform program and the American revolution shattered that prosperity and harmony. Most readers will accept these judgments, which, if unsurprising, are nonetheless significant.

Crane is at her best in describing and analyzing Newport's trade. Although the data do not lend themselves to rigorous quantitative analysis, the author's reassertion of the old view that slave cargoes and rum smuggling were major factors in the town's commercial prosperity is convincing. Anglicans, Congregationalists, Quakers, and Jews, future loyalists and future patriots, she explains, all had a hand in it. Although few if any engaged primarily in slaving, the spectacular profits from the triangular trade fueled much of the town's economy.

The author's social and demographic analysis of Newport's people and the impact of revolutionary events on their lives is mostly confined to the presentation of aggregate data drawn from tax lists and censuses. From these we learn that, in the generation before independence, the adult white female population rapidly outpaced its male counterpart, presumably because of the mortality and outmigration associated with marine occupations. The war accelerated this trend as patriot and loyalist men left women behind to care for family property and as the campaigns themselves took their toll. The consequences are mostly unexamined except for female poverty and a possible increase in prostitution. The black population (mostly slave) peaked by midcentury, at 18 percent, making Newport the most biracial town in New England. Crane presents little information on the largest segment of the society, poor and middling whites.

Politics, both internal and external, are largely by-passed by this study. The author does, however, explain Newport's prerevolutionary internal tranquility as the consequence of deference to a successful merchant oligarchy, and the divisions of the revolutionary period are attributed to differing judgments regarding economic self-interest and the likely outcome of the conflict (pp. 140–41). Although Crane notes that most wealthy Anglicans were loyalists and that most Whigs were not Anglican, she is reluctant to associate Newport's political conflicts with ideological, religious, or ethnic perspectives. Perhaps because her chief interest is the economy, she tends to treat all issues, such as abolition and women's rights, in economic terms.

Ultimately, Crane's account suggests that Newport was not only dependent on the sea but that its prosperity was also linked to the British empire. Although the sea remained economically important after the war and towns such as New London, Connecticut, Salem, Massachusetts, and Portsmouth, New Hampshire—as well as Providence, Rhode Island—recovered and flourished, Newport languished. Evidently, the loss of imperial advantages and entrepreneurial talent, connections, and capital all plunged Newport into decline even before

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it ran up against the limitations of its hinterland. Crane's study informs and illuminates these issues.

RICHARD D. BROWN
University of Connecticut

THOMAS M. DOERFLINGER. *A Vigorous Spirit of Enterprise: Merchants and Economic Development in Revolutionary Philadelphia*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture, Williamsburg, Va. 1986. Pp. xvi, 413. $32.00.

To write the history of a single mercantile firm in the field of foreign trade, as I can attest, is a complex and difficult task. To reconstruct the life of an entire mercantile community so engaged is almost incredibly more difficult. But Thomas M. Doerflinger has done that and even more. In a masterly study that should serve as a model not only for economic and business historians but also for students of the subtle interplay between economic development, values, change in social structure, and the tensions governing relationships between private and public life, Doerflinger has traced the changing contours of the Philadelphia mercantile community during the American revolutionary era—with penetrating glances both backward and forward in time.

Doerflinger knows exactly how foreign commerce was conducted; he divides Philadelphia's merchants between the dry goods and provisions trades, tells how incoming cargoes of cloth, hardware, and so on from England were financed and distributed throughout Philadelphia's extensive rural trading area, and explains how exports of bread, flour, meat, lumber, and flaxseed to various parts of the Atlantic world were conducted. The typical Philadelphia firm was fairly small and, in its choices of foreign destinations, specialized, preferring planned, routine voyages to more speculative, experimental ones. In correlating size with degree of geographical specialization, Doerflinger corrects a venerable misconception, including my own, that the typical merchant was a jack-of-all-trades who specialized in none. Even large-scale merchants specialized on particular ports and kinds of goods for extended periods, especially if specialization is measured by investment proportions.

Doerflinger's main thesis is that the development of the economy owed a great deal to those venturesome merchants who, provoked by adversity, dared to assume the risks not only of "speculative, experimental" voyages—for example, to China after the revolution—but also of investments in banking, securities, land, and manufacturing enterprises. Adversity was relative: hampered by capital shortages and small, highly competitive markets, northern merchants could not earn "the big profits reaped by the primary producers of the South and the Caribbean." This harsh economic climate led them to diversify into such areas as banking and manufacturing—enterprises that "eventually . . . launched the Industrial Revolution in the North" (p. 36).

Economic development also owed much to the potentiality of social mobility. Frequent failures, together with continual expansion in the size of the merchant community, "always generated new openings for would-be entrepreneurs," so that "risk and opportunity fed on each other in a symbiotic relationship" (p. 158). In contrast, southern agriculturalists, lulled into riskless complacency by the rising demand for cotton that was set in motion by the industrial revolutions in England and the United States, failed to diversify, developed "entrepreneurial anemia," and stagnated. In quantitative terms the region grew, but it failed to develop.

The distinction between growth and development is a familiar one, as is emphasis on the importance of entrepreneurship. In linking entrepreneurship with the prod of adversity and the lure of social mobility, however, Doerflinger contributes valuable insight that is worthy of test in other times and places. One sobering caveat: although businessmen may be provoked by adversity into innovative risk taking, they prefer nothing more than to minimize risk in a profitable environment. Not business but government must always be looked to for the maintenance of competition.

STUART BRUCHEY
Columbia University

DON HIGGINBOTHAM. *George Washington and the American Military Tradition*. (Mercer University Lamar Memorial Lectures, number 27.) Athens: University of Georgia Press. 1985. Pp. xii, 170. $16.50.

What is the American military tradition, and what can the career of George Washington tell us about it? Don Higginbotham's cogent answer may surprise those who think of Washington's Fabian generalship as a footnote to the American dedication to annihilationist warfare. For the tradition, as Higginbotham defines it, centers less on strategy than on the constitutional nature of the American military—specifically, on the republican heritage of civilian control. By exploring the tensions between Washington's military professionalism and his politically principled submission to civilian authority between 1754 and 1783, and by a judicious comparison between Washington's career and that of George Catlett Marshall, Higginbotham argues for Washington's centrality in the American military tradition.

As a young Virginia colonel in the French and Indian War, Washington showed the courage, effi-

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ciency, and professionalism that would mark his revolutionary career; but he was not particularly submissive to his civilian superiors. His legislative experience (1759–74), however, gave him a wider perspective on military issues than the one he had developed as a field officer. Higginbotham argues that Washington's political career, as much as his military experience, prepared him for command in the Revolutionary War. By carefully balancing military necessity against civilian sensibilities, Washington managed to hold the army together throughout the war, without forfeiting public or political support. Always the man in the uncomfortable middle, Washington demanded professionalism, insisted on the best treatment Congress could give his soldiers, and never swerved in his obedience to civil authority. Thus he not only prevailed in a long, wasting war but also "proved that a professional army was not incompatible with civil liberty" (p. 104).

In his last chapter, comparing Washington with Marshall, Higginbotham makes his strongest case for the vitality of the American tradition of civil supremacy, even in the age of the military-industrial complex. No less than Washington, Marshall bridged "the gap between the civilian and the military" (p. 138). As a professional and politically astute officer, Marshall understood civilians (as the equally professional and astute Douglas MacArthur never did), respected them, and willingly accepted their direction.

This publication is the twenty-seventh volume in the Lamar Memorial Lecture Series of Mercer University—a forum in which previous lecturers have produced distinguished works of synthesis and creative speculation. None, however, has excelled Higginbotham in vision, humanity, and wisdom: qualities amply demonstrated in this slender, eloquent volume.

FRED ANDERSON
University of Colorado

BARBARA CLARK SMITH. *After the Revolution: The Smithsonian History of Everyday Life in the Eighteenth Century.* New York: Pantheon, of Random House, for the National Museum of American History, New York. 1985. Pp. xxvi, 214. $24.95.

This handsome volume skillfully presents everyday life in the new nation. Barbara Clark Smith uses the lives of four men and their families, all of whom lived in the last quarter of the eighteenth century, to focus on daily experience. The book is in part the outcome of a new exhibit at the Smithsonian Institution, which uses house interiors that once belonged to these men and artifacts and graphics from their times to show the routine as well as the larger meaning of their lives. Both the exhibit and the book emphasize diversity and complexity in the experience of late eighteenth-century Americans.

Smith relates the history of these four families in the context of a larger environment. We learn of the raid on Samuel Colton's store in Long Meadow, Massachusetts, in 1776 both as an effect of the revolution and as a complex social event in an economy not yet geared to a totally commercial outlook. Life in Thomas Springer's log house in postrevolutionary Delaware was similarly tied both to external markets (in his case for grains) and neighborly exchanges. The Virginian, Henry Saunders, made his living from the cultivation and marketing of corn and wheat in an area that was giving up tobacco. Finally, Richard Allen, a slave who converted his master to Methodism and thereby obtained permission to earn his freedom, became a Methodist leader as he faced the difficulties of making his way as a freedman. In reading of these families, we learn of changes in the northern and southern agricultural economies, of the effects of evangelical religion, of the realities of slavery and struggles of slaves for freedom. Smith has read much of the most recent literature, some of it not yet in print, and has used it well.

Tying a general interpretation to the artifacts that a museum happens to have at hand—in this case three house interiors—presents problems. Recent discoveries suggest that Colton may never have occupied the Long Meadow house used to interpret his life style. More importantly, Saunders does not well represent a typical tobacco planter. He marketed corn and wheat in part because tobacco grown in Isle of Wight County was poor in quality, a fact that is not noted. Nevertheless, this book is a great success that should reach a general public, although its price may prove prohibitive.

LOIS GREEN CARR
Historic St. Mary's City

LOUIS C. HUNTER. *A History of Industrial Power in the United States, 1780–1930.* Volume 2, *Steam Power.* Charlottesville: University Press of Virginia, for the Hagley Museum and Library, Greenville, Del. 1985. Pp. xxii, 732. $50.00.

This is the second and, owing to Louis C. Hunter's death in 1984, final book of a projected three-volume set. The first volume, *Waterpower* (1979), received the Dexter Prize of the Society for the History of Technology. The present volume, although reminiscent of the author's classic *Steamboats on Western Rivers* (1949), extends the analysis of America's steam revolution in new and interesting ways.

Given its length (nearly seven hundred pages) and encyclopedic coverage, this is not the sort of

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book that most people will read from cover to cover. Hunter's discussion ranges from the earliest importations of low-pressure steam engines from Britain in the late eighteenth century to the development of large, sophisticated compound engines and steam pumps a hundred years later. Although he does not ignore applications of steam power to transportation systems, Hunter's primary emphasis is on stationary engines for manufacturing purposes and urban waterworks. As a result, he concentrates on the work of Oliver Evans (high-pressure steam used expansively), George H. Corliss (automatic variable cutoff engines), Charles T. Porter (high-speed mill engines), Edwin Reynolds (triple-expansion pumping engines), and Henry R. Worthington (direct-action pumping engines). The result is an instructive reconnaissance of steam-power engineering in the nineteenth century.

Although Hunter focuses on the design and deployment of steam engines, his book's most interesting sections reconstruct the shop practices employed in the manufacture of steam engines and boilers. Based on information gleaned primarily from nineteenth-century government surveys and census reports, these discussions punctuate the book at several intervals and serve to orient the reader to the larger industrial implications of steam engineering. One consequently learns a good deal about the technical equipment and commercial organization of such leading engine-building establishments as Evans's Mars Works in Philadelphia, Henry Latrobe's shop in Pittsburgh, and the Novelty Iron Works in New York City. Hunter argues convincingly that few if any shops specialized solely in the production of steam engines before the 1850s. Rather, "engine building was an occasional activity incidental to the production by foundries and machine shops of millwork, other heavy ironware, and machinery" (p. 233).

Even Evans's famous Mars Works, best known for its high-pressure engines, advertised its willingness to engage in a wide variety of millwork and machinery. Hunter is even more impressed by the number of small, nondescript shops with eight to ten employees and limited equipment that produced quality steam engines and other metal goods. Contrary to popular belief, he concludes that handicraft methods, rather than sophisticated mechanical techniques, dominated engine building and general machine work before the Civil War. One comes away from the book with a heightened appreciation of the role unschooled craftsmen played in America's first Industrial Revolution.

Although no central thesis dominates this study, a number of important themes can be discerned. One of the most interesting is Hunter's observation that early steam engines were adopted by manufacturers not because of engine efficiency or fuel economy but because they lightened work "in such simple but laborious operations as grinding, pounding, hoisting, pumping, rolling, and the like" (p. 170). In short, "engineering efficiency and commercial efficiency did not go hand in hand" (p. 507).

Another important theme is that scientific knowledge had little to do with steam engineering during most of the nineteenth century. Indeed, Hunter argues that even the best engines, such as Corliss's variable cutoff design and Porter's high-speed design, were "the products of inspired tinkering," not scientific theory (p. 435). Hunter also shows that the introduction of steam engines in the United States "was far more gradual than is frequently assumed" (p. 176). He thus finds it impossible to bestow the mantle of innovation on a single heroic figure, as earlier scholars tended to do with James Watt. Instead, he quotes approvingly from Robert H. Thurston's *A History of the Growth of the Steam-Engine* (1878) that "every great invention is really either an aggregation of minor inventions, or the final step of a progression. It is not a creation, but a growth" (p. 162). For Hunter, the history of the steam engine epitomizes technology as progress. In this respect his study has a celebratory quality reminiscent of many nineteenth-century treatises on the subject.

This book has many strengths, not least of which is the amount of fresh documentation that Hunter brings to the subject. Yet, as solid as the research is for steam engines, other peripheral areas receive less-than-adequate treatment. When discussing the origins of the machine-tool industry, for example, Hunter assigns far too much credit to the textile industry and not nearly enough to firearms manufacturers. This is so because his bibliography on such subjects is dated. Indeed, one looks in vain for secondary works published after the early 1970s. As a result, Hunter fails to take into account the outpouring of research that has appeared recently on urban waterworks, the machine-tool industry, the anthracite coal industry, and the "American system" of manufacturing. His account suffers accordingly. Finally, Hunter's technical descriptions frequently lack clarity and precision and are not as well coordinated with the illustrations as they should be. This makes for frustrating reading for someone who is interested in the technical details of steam-engine development. These shortcomings notwithstanding, Hunter has fashioned a fitting monument to the steam engine in America and to himself as a leading interpreter of the nation's Industrial Revolution.

MERRITT ROE SMITH
Massachusetts Institute of Technology

JAMES R. GIBSON. *Farming the Frontier: The Agricultural Opening of the Oregon Country, 1786–1846*. Seattle:

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University of Washington Press. 1986. Pp. 265. $25.00.

In recent years the historiography of the frontier experience has been enriched by the contributions of several prominent historical geographers. Arthur J. Ray and David J. Wishart in particular have done much to refine our understanding of the western fur trade. Social science methodology and spatial sensitivity have brought reinterpretations of the Indian's role in the fur trade and the physical setting of pioneer commerce. Any short list of historical geographers involved in frontier studies would also have to include James R. Gibson. In *Feeding the Russian Fur Trade* (1969) and *Imperial Russia in Frontier America* (1976) he described how eighteenth- and nineteenth-century Russian fur traders maintained posts in the hostile arctic environments of Kamchatka and Alaska. The challenges of supply, transportation, and procurement represent major themes in northern and western frontier history. On these themes, Gibson offers another monograph on the geography of supply in the North Pacific Basin.

Gibson's latest book concerns the agricultural opening of the Oregon Country. He deals with the seldom-considered period before the boundary settlement of 1846. Although his discussion of the settlements of the Willamette Valley is somewhat truncated, Gibson does a marvelous job of telling the story of the role of the Hudson's Bay Company in the agricultural development of the Pacific slope. The company initially developed garden plots at its fur posts as a means of reducing the cost of shipping supplies over the Rocky Mountains or around Cape Horn. The harvest of those plots did much to sustain trading posts that chronically ran short of rations and, in all cases, provided a welcome relief from the monotony of a salmon diet—which was the pemmican of the Pacific Northwest. So successful were these agricultural efforts that in 1839 the Honourable Company committed itself to commercial farming and livestock raising. The Puget Sound Agricultural Company was organized to encourage loyal British settlement of the Oregon Country and produce foodstuffs for sale to the Russian-American posts farther north. Ingeniously the company sought to increase its agricultural commitments in the west at the very time the Oregon Country's potential for fur trade was in decline. Surplus crops would then be used to barter for Alaskan furs from the poorly provisioned Russians. Gibson demonstrates that farming was not incompatible with the fur trade; in fact, it could be a complementary pursuit. Unfortunately for the Hudson's Bay farmers, the firm's governor, Sir George Simpson, typically tried to effect this grand scheme with as little expense as possible. The Puget Sound Agricultural Company did not attract many farmers from the Red River Valley, and those operations that were in place were dwarfed by the production of American farmers who flooded the Willamette Valley after 1843.

Gibson has written a fine monograph on the Hudson's Bay Company's involvement in agriculture during the last years of its regime in Oregon. I would only quibble with his rather cryptic concluding comment that "Canada has an irredentist legacy, to which the Oregon 'compromise' was the chief contributor" (p. 205). Although it is useful to draw broader implications from detailed studies, Gibson's own well-presented evidence argues that the failures of the Hudson's Bay Company made inevitable Britain's decision to concede Puget Sound to the United States.

THEODORE J. KARAMANSKI
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Chicago, Illinois

ANDREW MASON PROUTY. *More Deadly than War! Pacific Coast Logging, 1827–1981.* (Modern American History.) New York: Garland. 1985. Pp. xxvii, 252. $30.00.

This book has a frightening title that is not very descriptive of the contents. They are important and describe an aspect of American forestry history almost absent from the conventional texts. Andrew Mason Prouty discusses the terrible safety problems of an industry that was dangerous, dealt havoc to many workers in the woods and in the mills, and yet is ignored by all except survivors. Many people know songs of the woods such as "The Jam on Garry's Rock" but seldom think that the song commemorates the death of a young woodsman. Other songs with even grimmer messages are also known; perhaps the most poignant is "Will I Ever See Springtime Again?" Researchers have discussed death and damage in the woods, on the drive, and in the sawmills but usually have dismissed it as a part of life, unpleasant, but also unavoidable.

Prouty rectifies this slurring of the past. He examines the available documentary evidence for western states, most of which had commissions on workmen's safety by the end of the first decade of this century and supplements this evidence with much material from the press, the courtroom, the claims of injured workers, legend, and his own experience. The result is horrifying. Thousands died, and even more were maimed seriously in terrible accidents. As the huge, tall trees of the West were felled, the machinery used to do the work grew larger, more powerful, and faster. Men working around this machinery were not quick enough, were often tired, and lacked safety devices to protect them, and they suffered in consequence. According to a sort of folk mythology, sudden death lurked for

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all, but each listener believed that it did not apply to him. Just as young soldiers in combat know that death is imminent but are able to sublimate the personal possibility, so did the foresters, with song, joke, and story.

Many illustrations and tables accompany the text, and several appendixes give the appalling evidence of the numbers of deaths attributable to these accidents. All aspects of the industry are covered. Prouty organizes the sections on specific states and aspects of the industry according to categories of technique and machine. The study is horrifying and sobering, but it makes a useful contribution to forest history. Unfortunately, it is presented in a form that, at first glance, resembles a lurid Sunday supplement. Readers should go beyond this aspect, however, read the evidence, sort it through themselves, and come to the conclusion that the reforms of workman's compensation, mandatory safety devices, and worker training all came too late. The story of safety in the woods and in the mills needs discussion, for the 1920s and 1930s provide a backdrop to modern industry.

According to the old story, whenever a river driver sank beneath the logs, his colleagues cried, "Grab the cant dog! It belongs to the Company." Perhaps this story was true; perhaps not. Certainly, men died and were hurt. The problem was not well known, except in the industry, and was not dealt with directly until state and provincial governments began to mandate change. Prouty discusses this story in an important book that deserves wide readership. The past was often extremely difficult, even though colorful. Historians need to recall both aspects.

DAVID C. SMITH
University of Maine

JANE B. DONEGAN. *"Hydropathic Highway to Health": Women and Water-Cure in Antebellum America.* (Contributions in Medical Studies, number 17.) New York: Greenwood. 1986. Pp. xx, 229. $35.00.

The health and hygiene movement that swept across much of America during the antebellum era has been a topic of great interest to historians in recent years. In this book Jane B. Donegan examines one important aspect of this movement: hydropathy, more popularly known as the water-cure. Using the state of New York, a center of water-cure activity, as the focal point of her study, the author examines the ways that hydropathy benefited women as both patients and physicians.

The opening chapters of the book provide a survey of the origins and early history of hydropathy in Europe and America. Donegan notes that hydropathy entailed much more than simply caring for the sick through the external and internal use of water. She argues that it was a far-reaching system that promoted a healthy diet, rest, exercise, and sensible clothing. In the broadest sense hydropathists "set about to inculate a program of right living aimed less at treating disease than at preserving health" (pp. 13–14). The author contrasts the hydropathic approach to sickness and disease with that of "regular" physicians who often engaged in "heroic" medical procedures such as bloodletting and the use of massive doses of drugs.

Drawing primarily from articles published in the *Water-Cure Journal* as well as the writings of leading proponents of hydropathy including Joel Shaw, Mary Gove Nichols, Rachel Brooks Gleason, James Caleb Jackson, and others, Donegan describes how hydropathy embraced a philosophy that enabled women to assert considerable influence over their own health and also allowed them opportunities to become physicians. This philosophy is carefully depicted in chapters that examine the hydropathic approach to pregnancy and childbirth. In contrast to orthodox practitioners who emphasized the "scientific management" of parturition, hydropathic reformers viewed childbirth as a natural physiological function that normally required minimal interference by the physician. Hydropathists were distinguished from their orthodox counterparts by their prescriptions concerning prenatal and postpartum care. Whereas most physicians paid little attention to the prenatal period, hydropathists stressed the importance of proper diet, weight control, ample rest, and moderate exercise in preparation for a successful delivery. By supporting ambulation and moderate exercise as soon as possible after birth, hydropathists again found themselves at odds with orthodox practitioners, who generally called for a recuperative period of one month or longer. In response to the demands of the new obstetrics and the desire to preserve the delicacy of the sexes, hydropathists also supported the unorthodox tactic of encouraging women to become physicians.

This is a thoroughly researched and well-written work on an important, but neglected, aspect of hydropathy. Donegan is to be congratulated for her judicious portrayal of both hydropathic reformers and orthodox physicians. Although she makes clear that the water-cure was not the panacea its proponents claimed it to be, she does demonstrate that hydropathy's emphasis on self-reliance and the preservation of health especially benefited women and was remarkably advanced for its time.

JUDY BARRETT LITOFF
Bryant College

MELVIN L. ADELMAN. *A Sporting Time: New York City and the Rise of Modern Athletics, 1820–70.* (Sports and

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Society.) Urbana: University of Illinois Press. 1986. Pp. 388. $24.95.

Challenging previous works that located the rise of modern sports in the late nineteenth and early twentieth centuries, Melvin L. Adelman argues that the institutional, commercial, and ideological character of modern sports was shaped in the period between 1820 and 1870. Focusing on New York City, the nation's sports capital during these years, he charts the transformation of a host of sports from their "premodern to modern pattern" (p. 2). The book is divided into three sections: an examination of turf sports, of ball games, and of a potpourri of other popular sports.

Adelman's impressive survey of nearly two dozen activities is enriched by his ability to see sports as something more than a mirror of society or a simple by-product of the process of urbanization and industrialization. Sports are institutions with their own history and dynamics. Although Adelman acknowledges the importance of external factors (changes in the city's demography, social structure and relations, popular ideologies, and so on), he also explores the internal developments (such as growth in popularity, competition, commercialization, and rationalization) that help explain why some sports thrived while others did not; why some remained amateur activities while others were professionalized. He shows why thoroughbred racing, initially one of the city's most popular commercial sports, lost out to trotting, a relative latecomer, as the king of turf and America's first truly modern sport. Similarly, in explaining why baseball superseded cricket as the city's and nation's most popular sport, Adelman stresses the importance of the differing internal structures of the two games. Baseball, with its rapidly alternating innings, was a faster-paced, more dramatic game for participants and spectators alike, and, because of this, the popularity, commercialization, rationalization, and professionalization of the sport were enhanced.

Whereas horseracing and baseball appealed to a wide range of classes and ethnic groups, yachting and racquets were dominated by elites. Running, billiards, boxing, and animal sports (cockfighting, rat baiting, and dogfighting), on the other hand, found their greatest advocates among the working classes and wayward sons of the elite. In one of his most interesting sections, Adelman shows how class, gender, and ethnicity interacted to foster the growth and commercialization of billiards and boxing. He also presents a sweeping portrait of noncompetitive and generally noncommercial sports (gymnastics, roller skating, croquet, and so on) that were popular with a wide range of New Yorkers. Adelman concludes by discussing the rise of the ideology of modern sports and the endeavors of various writers to resolve the tensions between older proscriptions against play and leisure and the development of sports as a widespread and often commercial enterprise.

This book operates at two levels: one describes the complex evolution of various sports while the other analyzes that evolution within the framework of modernization theory. The first succeeds admirably; the second does not. I find most modernization theory seriously flawed, and this work is no exception. Distinctions between premodern and modern are often forced and based on reading our knowledge of subsequent development back into the past. Sports that assume the characteristics of contemporary commercialized athletics are "modern" (distinguished by standardized and institutionalized organization, rules, competition, role differentiation, dissemination of public information, and keeping of statistics and records); those that do not are "premodern." The attempt "to apply modernization to the history of sport" (p. 4) often mars an otherwise fine analysis. The book would also benefit from a more thorough discussion of the relationship between unorganized and organized sport and an examination of sports as part of a range of voluntary associations that were undergoing major changes between the 1820s and 1870s. Despite these reservations, Adelman's extensive analysis of a previously underexplored period of sporting life and his close examination of the internal dynamics that shaped the evolution of various popular activities make this work an important contribution for sports and social historians alike.

STEVEN J. ROSS
University of Southern California

KENNETH S. GREENBERG. *Masters and Statesmen: The Political Culture of American Slavery.* (New Studies in American Intellectual and Cultural History.) Baltimore: Johns Hopkins University Press. 1985. Pp. xii, 195. $22.50.

BERTRAM WYATT-BROWN. *Yankee Saints and Southern Sinners.* Baton Rouge: Louisiana State University Press. 1985. Pp. xi, 227. $20.00.

Both of these books reemphasize fundamental differences between North and South as the cause of the Civil War. Rather than stressing disputes over slavery or its expansion, however, they revive the notion of distinctive civilizations that possessed contrasting and conflicting values. Thus, both implicitly reject my belief that a common commitment to republican ideology ultimately led to war.

Contending that recent historians have improperly minimized the cultural differences between the sections, Bertram Wyatt-Brown attempts to demonstrate "the great moral dichotomies that led to

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secession and war" (pp. 4–5) in a collection of essays, all but one of which have been previously published. Four of the essays focus on Yankee Saints, the northern abolitionists; three deal with the South, including the new essay, "Honor and Secession." Each essay merits individual attention, but Wyatt-Brown's overall thesis is of moment here.

At the core of the antislavery movement, and, by extension, of northern culture in general because abolitionism was "eventually, though imperfectly, incorporated into the northern or at least the Republican system of values," stood a religious impulse, evangelical Protestantism (p. 29). Its stress on individual conscience, rather than external opinion, as the arbiter of behavior and its aim "to establish moral uniformity throughout the land" defined a distinctive northern ethic (p. 10).

In contrast to the religiously based northern culture, southerners adhered to a traditional, secular ethic of honor that was inseparable from the practice of slaveholding. Unlike evangelical Protestantism, honor prized public reputation, masculine assertiveness, and conformity to community standards and traditions. Violations of a Christian conscience evoked guilt and an impulse toward personal and societal reform; challenges to honor provoked shame and a demand for vindication. Honor caused southerners to understand liberty and equality differently from northerners. To southerners liberty meant freedom from state coercion, especially in matters regarding slavery and race, not individual freedom from sin. Yet liberty meant more than freedom from enslavement. Liberty involved "the racially elitist concept that all white men were equal in their hegemony over blacks" (p. 203). Rejecting northern demands for moral uniformity and political rule by the northern majority, southerners demanded sectional parity, respect for their political and moral equality. According to Wyatt-Brown, northern insistence that the South was politically and morally inferior wounded southern honor and aroused the indignation that, more than anything else, provoked secession and war.

Each essay is perceptive and suggestive. Yet one can question whether the religiously inspired abolitionists were representative of the entire North and thus whether religious values explain the increasing northern animus toward the South. Wyatt-Brown himself appears to share this doubt in his description of the northern reaction to John Brown's raid and of the desire for power that guided the Republican party. It is also not clear, as the studies of J. Mills Thornton and Marc Kruman among others show, that southern nonslaveholders' concept of liberty was inseparable from the right to hold slaves and degrade the black race. Much of the southern rhetoric and behavior that he ascribes to honor, indeed, can also be understood in terms of an alternative republican value system that northerners shared. The extent of the ethical differences between North and South and the degree to which those supposed moral dichotomies caused the Civil War, in sum, remain unclear.

Whereas Wyatt-Brown installs honor alone at the center of southern culture, Kenneth Greenberg argues that slavery engendered a unique political culture that fused honor and republicanism, a term Wyatt-Brown never even mentions. The tension between the need to exercise power over others that is inherent in honor and the fear of power and its corruption that is inherent in republicanism, Greenberg insists, shaped not only political attitudes but also the master-slave relationship. Thus the experience of slaveholding and other southern practices such as the duel paralleled and reinforced southern beliefs about the proper political relations among whites. The central problem of white southerners, he maintains, was to avoid becoming either an enslaver who abused power or a slave who was subject to it. The attempt to resolve this problem caused southerners to cling to ideals of virtual representation and disinterested, nonpartisan statesmanship longer than northerners, rendered political parties and popular attachment to them far weaker than in the North, and generated a defense of slavery that denied that the institution involved the exercise of absolute power over others.

The combination of honor and republicanism also shaped the South's response to sectional conflict. Greenberg makes the intriguing point that during the antebellum period southerners transferred intact their fears of England as an enslaving despot to the North. Secession was motivated by both republican fears of enslavement and honor's demand that insult and inferiority be resisted. More than this, secession was an attempt at purification, an attempt to escape a corrupt, spoils-oriented politics that southerners identified with the North and to revive the long-cherished ideal of disinterested statesmanship for the common good.

Greenberg's argument is coherent, imaginative, and often ingenious. Yet it remains uncompelling because he has not demonstrated that the political culture of slavery was either characteristic of the entire South or different from northern political values. Most of his evidence is drawn from South Carolina. Although he admits that the state was unique in some respects and that not all southerners embraced its values, he tends to treat South Carolina as archetypal rather than atypical of the region and insists that the political culture of slavery dominated Dixie. If the master-slave relationship was its key ingredient, however, one wonders about the political values of the nonslaveholding majority. Along the same lines, he has described the values of the political elite, or at least part of it, but he has said

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nothing about the concerns and motivations of average voters who never aspired to office and who, despite the competitive imbalance between parties in some southern states, participated in elections and other political activities with the same zeal as northerners. Did ordinary, nonslaveholding voters share the same political values as their slaveholding leaders? Similarly, although Greenberg repeatedly admits that the combination of elements, rather than any single aspect, made the political culture of slavery unique, he can only prove that case through a systematic comparison with the North, an analysis he eschews. As a result, the distinctiveness of southern political culture is asserted, not proved.

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ROBERT F. DURDEN. *The Self-Inflicted Wound: Southern Politics in the Nineteenth Century.* (New Perspectives on the South.) Lexington: University Press of Kentucky. 1985. Pp. x, 150. $16.00.

This interpretive essay brings a distinctive perspective to a familiar subject. Surveying the course of southern politics from 1800 to 1890, Robert F. Durden presents a comprehensive interpretation of that history. The essay generalizes on a high level but grows out of Durden's previous monographic studies.

Durden depicts a South that "went wrong." He emphasizes the positive character of southern politics in the Jeffersonian period and makes the most of the contemporary southern criticism of slavery. In every subsequent period, he finds, the defense of slavery (and, later, white supremacy) stifled democratic and humane impulses in regional politics. That "tragic" distortion turned the early "optimism and nationalism" into "sullen, defensive sectionalism." The change stemmed mainly from "a self-inflicted wound": "the gradual surrender of the southern white majority . . . to the pride, fears, and hates of racism" (p. 132). The essay traces that "surrender" from the Missouri statehood controversy through Reconstruction and Redemption.

In his account Durden balances national controversies with the history of parties and issues within the South. In only 132 pages he can give some topics only "textbook" treatment. Coverage of state politics is slight, and the "gag rule" escapes attention. On the other hand, Durden discusses some topics in depth. He calls attention to the neglected Ohio compensated-emancipation plan of 1824. He closely scrutinizes the Confederate slave-enlistment discussion of 1864–65 and the Readjuster movement in Virginia. His account of Reconstruction focuses on northern policy making more than on politics in the South, and the apparently arbitrary terminus in 1890 precludes discussion of Populism and disfranchisement laws.

In addition to the main thesis two features characterize the narrative. First, Durden's history of the sectional conflict appears biased in favor of such moderates as Stephen A. Douglas. Second, Durden gives a maximum estimate of the influence of racial prejudice in American politics. The relation between the two features is not clear. One may argue that racism was explicitly more central to Douglas's sectional moderation than to the policies of his antislavery and proslavery opponents.

The self-inflicted wound interpretation, anticipated by some previous writers, deserves discussion. Its most problematical aspect lies in abstracting the defense of slavery (and white supremacy) from the political tradition that the defense allegedly corrupted. The studies of William J. Cooper, Jr. suggest closer congruence between the elements that Durden sets in mutual tension. Commitment to defend slavery, when necessary, was not a novelty that entered southern politics after 1800, and engagement in sectional defense did not keep southern politicians from making positive contributions to the region and nation throughout the nineteenth century.

JACK P. MADDEX, JR.
University of Oregon

MERTON L. DILLON. *Ulrich Bonnell Phillips: Historian of the Old South.* Baton Rouge: Louisiana State University Press. 1985. Pp. xiii, 190. $20.00.

When Ulrich Bonnell Phillips died at age fifty-six in 1934, his reputation as one of the foremost academic historians of the twentieth century seemed secure. Acclaimed for his pioneering work in southern social and economic history, for his pathbreaking studies concerning the institution of slavery, and for his discovery and collection of hitherto-unknown plantation records and manuscripts, Phillips was widely credited with being to the history of the antebellum South what Frederick Jackson Turner had been to the western frontier. Yet, within a generation both the man and his works had been either forgotten or consigned to the limbo of courses in American historiography. Interest in and appreciation of Phillips was revived in the 1960s and 1970s largely through the efforts of Eugene Genovese who rightly perceived that, notwithstanding his racism and romanticization of the planter aristocracy, Phillips was a first-rate historian and a true innovator. In this brief but fine biography Merton L. Dillon provides an informed and judicious assessment of Phillips and his writings.

The author makes clear that Phillips was a product of the times in which he lived and that his

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scholarship was shaped accordingly. Born in Georgia, he reflected a southern way of thinking as well as a predisposition to see the benign aspects of master-slave relationships. Greatly influenced both professionally and personally by Frederick Jackson Turner, he was a confirmed Progressive who believed that history should be a useful tool for reform. After Phillips received his Ph.D. under William A. Dunning at Columbia, Turner brought him to Wisconsin. He moved to Tulane for a brief period, then was lured to the University of Michigan where he spent the larger part of his career. He left Ann Arbor in 1929 and spent the last four years of his life at Yale. Dillon does not overlook Phillips the academician. In fact, the book offers some revealing insights into the nature of the profession and its politics, including Phillips's role in the abortive revolt of 1915 against the American Historical Association.

Dillon, however, does not become sidetracked. Emphasis is on Phillips the historian. He provides a thorough discussion of *American Negro Slavery* (1918) and *Life and Labor in the Old South* (1929) along with a fascinating chapter on Phillips's manuscript hunting. The book is not an apologia but a skillfully crafted, carefully researched, and gracefully written work that convincingly demonstrates that historiography can also be history at its best.

TIMOTHY P. DONOVAN
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JEAN E. FRIEDMAN. *The Enclosed Garden: Women and Community in the Evangelical South, 1830–1900*. (Fred W. Morrison Series in Southern Studies.) Chapel Hill: University of North Carolina Press. 1985. Pp. xvi, 180. $19.95.

For a volume so tranquilly entitled, Jean E. Friedman's book promises to spawn diverse opinion, to generate praise, to give pause, and to enlist defenders and detractors aplenty. Friedman's ingenious and ambitious book searches the mind, church, and community of the South in an effort to explain why the southern women's reform movement lagged behind that of the North. Friedman's explanations are peppered with new ideas, keen insights, and controversial conclusions based on unconventional sources. Future research may or may not substantiate these conclusions, but Friedman has undoubtedly charted new paths and has done so with aplomb.

Traditionally historians have focused on slavery to explain the quiescent southern woman. Although she does not discount slavery as a factor, Friedman delves into the complex structures of southern society and uncovers deeper explanations. Women in the preindustrial South, she contends, were products of their society—a society described as a tightly knit evangelical community directed by church and dominated by kinship networks. Connections of family and kin and the discipline of church defined human relationships in the agrarian South. The deeply rooted familial structure inhibited gender group expression; female associations did not evolve; and masculine control of pulpit and punishment in local family churches ensured strict adherence to traditional sex roles. In the North rapid modernization made for increased segregation of the sexes, and the evangelical movement prompted a proliferation of women's societies that created a sense of collective identity and a social basis for reform. The southern community allowed no such opportunity, and women remained without a base from which to challenge the patriarchy.

Friedman finds a similar evangelical kin connection in the southern Afro-American community. Yet the unique Afro-American culture, the family-kin bonds, and the workings of the peculiar institution allowed black women to interpret their roles differently and to assume greater autonomy within their community than did their white counterparts.

The author also attends the expectations that the evangelical social structure demanded of women in marriage, home, field, and town. In each case women carried out responsibilities without corresponding autonomy, in accord with the dictates of church and community. That the preindustrial community demanded maternity, domesticity, self-denial, and piety from women is certain; that these communal expectations initiated anxiety, frustration, and resentment in many women is clear; that women most frequently responded by beating their breasts in self-denigration when they failed to meet these expectations is incontestable; but that dream analysis provides best evidence is most dubious. The inherent subjectivity of such analysis does little to enhance the credibility of Friedman's thesis, and the psychological jargon clouds meaning and makes for murky prose. The author has established a solid argument without probing the subconscious.

Friedman's treatment of the southern community during the Civil War and the postwar era is equally convincing. Not even the disruption of war altered traditional familial and kin patterns. Only with the onset of modernization in the late nineteenth century did conditions allow the bare beginnings of a collective sense of identity and the first stirrings of a women's rights movement. Even then, southern women proceeded in orthodox fashion. By supporting temperance, missionary work, and white supremacy, they rendered reform respectable because they stressed femininity first and reform second.

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Unquestionably, this book is a first-rate contribution to the historiography of women in the United States.

CAROLYN E. DELATTE
McNeese State University

EDWARD G. LONGACRE. *The Cavalry at Gettysburg: A Tactical Study of Mounted Operations during the Civil War's Pivotal Campaign, 9 June–14 July 1863*. Cranbury, N.J.: Associated University Presses, for Fairleigh Dickinson, Rutherford, N.J. 1986. Pp. 338. $39.50.

Edward G. Longacre's stated purpose is to plug one of the few remaining gaps in the literature of the Gettysburg campaign by describing the tactical contributions made by the mounted arms of the Army of Northern Virginia and the Army of the Potomac between June 9 and July 14, 1863. Longacre accomplishes his task well, in so far as it goes. He emphasizes the tactical level and here is superb. The book is thoroughly researched and includes an extensive bibliography of manuscript collections, some hitherto unmined. No detail of even minor mounted skirmishes is overlooked. Any future researcher wishing to know the specifics of cavalry operations in the campaign will start here.

The book's strength, however, is also its weakness. Longacre is so concerned with detail that we often lose sight of the wood for the trees. His feel for the subject falters when he moves from tactics to strategy or, put another way, from the small to the big picture. A claim in the preface "that the horse soldiers of North and South shouldered the major burden of the campaigning" fails to be demonstrated in the text (p. 10). Certainly, the cavalry got a lot of work, but at no point do we feel that the mounted men's role was decisive. Even Jeb Stuart's failure to be with Robert E. Lee at the crucial height of the campaign appears to be of secondary importance.

Perhaps the difficulty is not so much with this volume, which makes a solid contribution, as it is with the whole genre of writing on the Civil War. Buffs provide a large sector of the market for books on the Civil War, and they have tended to prefer works that are heavy on narrative and facts and light on theory and speculation. Thus, historians of the Civil War often tend to be behind the times. They ignore insights from other disciplines, such as psychology, and are oblivious to the new approaches of, say, feminist scholars. Descriptive language becomes hackneyed. We now know that horses avoid obstacles and refuse to hit each other, but in histories of the Civil War cavalry still crash into each other with regularity. It is time for the genre to catch up to the discipline as a whole.

I do not wish to take the point too far. This is a good work and will be enjoyed by the many devotees of the war.

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IRA BERLIN *et al. Freedom: A Documentary History of Emancipation, 1861–1867*. Volume 1, *The Destruction of Slavery*. (Selected from the Holdings of the National Archives of the United States, series 1.) New York: Cambridge University Press. 1985. Pp. xxxviii, 852. $39.50.

In 1976 Ira Berlin began a systematic search of the immense Civil War records of the National Archives. He and his associate editors, Barbara J. Fields, Thavolia Glymph, Joseph P. Reidy, and Leslie S. Rowland, sought a documentary record of the "processes . . . central to the transition from slavery to freedom" (p. xxi). Selections from the forty thousand items that they identified, thoroughly annotated, and ably introduced appear as the multivolume *Freedom: A Documentary History of Emancipation*. The introductory volume of that history is the subject of this review.

On one level *Freedom* is a forum in which enslaved people speak and act as agents of their own liberation. This volume records individual struggles for freedom, the brutal but often futile efforts of masters to maintain control over their human property, and the uncertain responses of Union forces to the growing population of "contraband" blacks within their lines. Particularly revealing is black testimony before the Southern Claims Commission in the early 1870s indicating that in the low country of South Carolina and Georgia some slaves owned substantial property, to the disbelief of the foraging Union soldiers who seized slave possessions as rebel property and to the surprise of the commissioners authorized by Congress to compensate southern unionists for property confiscated or destroyed by the Union army.

Striking as well are the efforts of black men in the border states to secure freedom for themselves and protection for their families by joining the Union army. By 1863 Union army recruiters enrolled able-bodied black men regardless of their slave status in the loyal states. The Union army thus offered an important avenue to freedom. As soldiers, black men developed a powerful sense of self-control as well. In a remarkable letter to his wife, a Missouri black soldier promised his protection and warned her master that black troops, having "labored in the field to Subdue Slavery," now intended "to protect" those not yet formally freed. The soldier's warning prefaced a specific threat: "Dixon I expect to be taken from his home and

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never return again." A letter purportedly written by a Union officer on behalf of the soldier also threatened Dixon with "armed force" if he failed to release the soldier's wife. The specific cause of the soldier's anger is not known, but the implications of the threats are clear. Described in a provost marshall's report as "a very kind & humane Master & a Gentleman—but of rather doubtfull loyalty," Dixon wrote to the federal commander of the Department of the Missouri protesting "meditated violence on the part of the Soldiers" (pp. 483–86).

Documents of this sort are extraordinarily valuable in part because they are rare. This soldier's letter, for example, is the only statement by a black person in the fifty documents depicting the process of emancipation in Missouri. In the entire volume only 12 percent of the documents record black testimony. Perhaps this paucity of statements from blacks will be atypical of the *Freedom* series as a whole. Certainly the previously published volume of Series 2, *The Black Military Experience* (1982), raised higher expectations. In that volume 37 percent of the documents represented testimony by black men and women. Nevertheless, the predominance of white sources—some of them available in the *Official Records of the War of the Rebellion* (1880–1901)—underscores the editors' intention to present their archival material in a broad interpretive history of emancipation in America.

The editors describe their work as differing "fundamentally from most historical editing enterprises." Their goal is to "combine the strengths of the traditional interpretive monograph with the rich diversity of the documentary edition" (p. xxii). In their interpretive role the editors insist that emancipation "accomplished a profound social revolution" by "forcibly substituting the relations of free labor for those of slavery" (p. xv). Later, when this substitution appears merely nominal, the significance of emancipation becomes profoundly subtle as well: "Instead of grappling with freedom on the terrain of slavery, they [the former slaves] now grappled with slavery on the terrain of freedom" (p. 341).

The specific analysis of slave labor relations and the postwar requirements of free labor relations suggested by these statements does not appear in the introductory essays or notes of this volume. Perhaps it is reserved for future volumes. In any case, the great strength of *Freedom*—its sustained concern with the manner in which individual "men and women strive to enlarge their freedom and secure their independence from those who would dominate their lives" (p. xxii)—is clear and unlikely to change. When the editors identify the specific experiences of emancipation with the distinctive character of slave communities, they deepen our understanding of the unique dimensions of American slavery and emancipation. Thus, conditions and experiences in the largely autonomous slave communities of the South Carolina and Georgia low country differed substantially from experiences in the more competitive labor markets of the border states, for example, or the isolated conditions confronting plantation communities in the Mississippi Valley.

As a documentary record and interpretive survey of American emancipation, *Freedom* is unlikely to be surpassed.

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DAN T. CARTER. *When the War was Over: The Failure of Self-Reconstruction in the South, 1865–1867*. Baton Rouge: Louisiana State University Press. 1985. Pp. xiv, 285.

Dan T. Carter's book describes the actions and attitudes of white Southerners, particularly white southern leaders, from the end of the Civil War until Congress took control of Reconstruction early in 1867. That period has been called a time of "self-Reconstruction," or "Confederate Reconstruction," since the policies of President Andrew Johnson allowed white Southerners (with a few restrictions) to adopt state constitutions, elect officials, pass legislation, and thus, in those respects, to govern themselves.

This book contains unusually full and detailed descriptions of people and events, supported by citations to numerous manuscript collections, newspapers, magazines, and state and federal records. From this voluminous evidence emerges a portrait of white Southerners more detailed and complex than in many previous accounts. Carter has set these rich descriptions in an interpretive framework that is explicitly "revisionist" in its modifications of the conclusions of some historians whom Carter names, including John Hope Franklin, Kenneth M. Stampp, Michael Perman, Eric Foner, and W. E. B. Du Bois. But Carter's disagreements with those named historians seem to be primarily a matter of "fine-tuning" and subtle shading, rather than outright rejection—a matter of describing white southern leaders of 1865–66 in terms somewhat less hostile than those used by the historians mentioned. Carter makes it clear, however, that he does "not come to praise" (p. 3) the white southern leaders, and he describes their self-Reconstruction as a failure.

Carter's mild and qualified revisionism consists partly in identifying white southern leaders differently from some earlier accounts. The majority of those postwar leaders had not been ardent prosecession fireaters during the crisis of 1860–61, we are

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told, but rather "reluctant secessionists who had initially opposed secession and then, when it was an accomplished fact, supported the new nation" (p. 3). A "surprising minority" of the leaders had "either been neutral during the war or had overtly opposed the Confederacy" (pp. 3, 27), and a "surprising number" of the leaders were "veterans of the old Whig party" (p. 4). They were not "an ideologically cohesive planter class, adamantly intent on resisting any change in their society," but were "cautious and conservative by temperament" and lacked "the cohesive ideology and self-identity that characterized nineteenth-century rural landed elites in other societies" (p. 4).

Matching Carter's differing identification of the white southern leaders from some earlier historians is his different description of the policies favored by the leaders. Their policies toward blacks were not "blindly reactionary" but were "at least within the context of white southern thought . . . distinctly conservative" (p. 3). With a respect to economic policies, a "substantial number . . . [of them] (including many planters) . . . endorsed tax and credit policies that would promote industrial as well as agricultural development" and "appeared ready to accept the destruction of the South's plantation economy and its replacement by an agricultural society dominated by more efficient small-scale farmers" (p. 4).

The space available does not allow a close analysis here of the evidence Carter advances in support of the propositions quoted above, and so I must present my evaluation of the book in general terms. That evaluation is mixed. On the one hand, I welcome the attempt to make more precise and more accurate our understanding of Reconstruction, and I have learned much from the book. I also applaud a restrained revisionism such as Carter's, which does not assert that it has made obsolete all earlier interpretations of its subject. On the other hand, this exercise in fine-tuning existing views of Reconstruction seems at times to be so fine as to be almost imperceptible—does Carter's description of the Black Codes of 1865–66, for example, alter substantially the account commonly accepted by many historians of Reconstruction today, whether one describes the racial policies as "blindly reactionary" or "distinctly conservative"? This book may make its prime contribution as a supplement and complement to views of Reconstruction current among historians today, more than as a new interpretive framework.

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DAVID VAN TASSEL and PETER N. STEARNS, editors. *Old Age in a Bureaucratic Society: The Elderly, the Experts, and the State in American History*. (Contributions to the Study of Aging, number 4.) New York: Greenwood. 1986. Pp. xx, 259. $35.00.

The fecundity of the new social history is nowhere more evident than in this invigorating compilation of articles focused on old age. They were originally presented in 1983 at a conference held at Case Western Reserve University whose purpose was to "encourage the continuing historical analysis of old age, to focus new research efforts on the problems of bureaucracy and social policy, and to bring the power of historical knowledge and analysis together in addressing the challenges posed today by the crisis of the welfare state" (p. xi). In a well-written and tightly constructed introduction, David Van Tassel and Peter N. Stearns, both established contributors in the field of old age, present, among other things, a survey of the history of the field, its shortcomings, and a prescription for future research. Even with such a formidable introduction one would have wished for at least a passing commentary, if not some analysis, of the concepts of old age and of what constitutes a bureaucratic society.

The essays are organized topically. Part 1 contains valuable articles by Brian Gratton and Gerald N. Grob, who comment on the recent historiography of old age. Gratton provides a critical analysis of the major historians and claims that historians have overemphasized attitude as the key ingredient for explaining the history of the elderly. Instead, Gratton proposes that greater stress should be put on structural changes in society, that is, the spread of industrialism. Although Grob agrees that structural change has been underestimated, he rejects what he states is Gratton's determinism in favor of recognizing randomness in the explanation of historical change. Part 2, containing papers by Thomas R. Cole and Carole Haber, discusses the etiology of attitudes toward the elderly and the impact, in turn, of these attitudes. Cole assumes the existence of ageism, or negative attitudes toward the elderly, but, apart from antitraditionalist material from evangelical tracts, he provides little direct proof of it. Even the circumstantial evidence represented by the tracts themselves constitutes an unpersuasive source of attitudes toward the elderly. In contrast Haber attributes nineteenth-century ageism to the emphasis on the degenerative qualities of old age popularized by medical sources. Along with this emphasis, Haber discovers a pervasive hopelessness that she claims persists today and that colors much of physicians' behavior toward older people. Although Haber's arguments appear convincing, Charles Rosenberg's incisive observation cautions that "the relationship between attitudes and behavior remains obscure" (p. 233).

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In part 3 Daniel Scott Smith and Tamara Hareven concentrate on changes in the circumstances of the elderly from the nineteenth to the twentieth centuries. Smith employs statistical analysis and Hareven uses qualitative methods; both agree that the twentieth century is innovative in changing the centuries-old preeminence of family care in favor of state responsibility for elderly affairs. They disagree, however, on whether the nineteenth-century family was predominantly nuclear: using census data, Smith concludes that it was not, whereas Hareven contends that it was. Such seeming conflict may well be resolved if household arrangements are investigated over the entire family cycle. Part 4 concentrates on socioeconomic policies toward the elderly. Jill S. Quadagno provides an excellent historical survey of the evolution of social policy toward old age from the seventeenth to the nineteenth centuries. The ensuing articles by W. Andrew Achenbaum and John Myles center on social security, its history, its current crisis, and the policy options now available. Achenbaum's carefully drawn history enables him to propose the solution of reining in benefits as a last resort, whereas Myles advocates increasing the input of revenue as part of a general effort to achieve material equality.

In Section 5 Rosenberg crafts a well-reasoned and stimulating essay, noting initially that "contemporary scholarship is marked by a tendency to use assumptions about . . . interrelationships in place of evidence and the critical building of middle-level theory" (p. 232). Instead of working with broad hypothetical constructs such as modernization, Rosenberg urges the use of "an approach anchored in real places and particular times"—his middle-level theory. Thus he delves into analysis of the medical system and its relationship to elderly patients and seeks to shed light on both the nature of the system and its impact on the old.

No brief review can touch all the significant contributions contained in this densely packed volume. Its publication marks a coming of age of the field, but, as the editors have noted, much remains to be done on the acquisition of data, analysis, and synthesis. No matter how extensive and thoughtful historical analysis may come to be, however, its relevance to policy development may be limited. Among the more successful uses of history is Quadagno's demonstration that poor law concepts have continuing vitality in current policy. On the whole, detailed analysis provides a rich store of insight and information for the specialist historian and nonhistorian alike. For the nonspecialist this volume contributes a valuable introduction.

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DONALD WORSTER. *Rivers of Empire: Water, Aridity, and the Growth of the American West.* New York: Pantheon. 1985. Pp. x, 402. $24.95.

Just fifty years ago, in 1937, Walter Prescott Webb wrote an angry book, *Divided We Stand,* in which he bemoaned the colonial status of the American West in relation to the older East. Webb was not alone in his anguish. It was reiterated by a chorus of other writers and by millions of westerners in all walks of life. As the American colonies had once been beholden to the mother country, so the agricultural and mining West was an underdeveloped area—America's Third World—paying tribute to the industrialized, capital-rich East. The development of this colonial theme in western history perhaps made inevitable an imperialist analysis of the growth of the twentieth-century West by a succeeding generation of critics, describing the establishment of a new empire west of the ninety-eighth meridian by corporations and the federal government in the second half of the twentieth century. This book by Donald Worster, an environmental historian, is a comprehensive effort to analyze the creation, evolution, and what he prophesizes as the impending fall of this newest American empire.

For his conceptual framework Worster turned to Karl Wittfogel's theory of hydraulic societies. Wittfogel, it will be remembered, attempted to fuse Karl Marx's analysis of economic class relations with Max Weber's emphasis on the influence of bureaucracy, particularly as these were reflected in Oriental despotisms such as China and India during the last four thousand years. Whoever controlled the water in these societies had absolute power over political, economic, and social institutions. Like most members of the "Frankfurt School" in the 1920s, Wittfogel was a severe critic of centralized state power and bureaucratic authoritarianism. That Weltanschauung is heartily endorsed by Worster, who extends Wittfogel's critique to the American West. As Worster sees it, control of water was the crucial element in western growth from the earliest days of settlement. Development of the region was primarily undertaken by corporate and governmental elites who, using wealth and technological skill, dominated political institutions, created patterns of economic development, and shaped the social structure. The contours of civilization in the trans-Mississippi West increasingly came to depend on dams, reservoirs, and canals built by corporate interests or the Bureau of Reclamation. Much of the book, in fact, is a severe indictment of the bureau and its frenzied effort to dam the West. No less than the pharaohs of ancient Egypt or the Han rulers of imperial China, Worster notes, the water elites have left their imprint on the region. As ancient despots built dams and pyramids, so the recent builders of

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the West have left dams and reservoirs as monuments to their acquisitiveness. Using this perspective, Worster analyzes selected aspects of western development since the middle of the nineteenth century, including Mormon policies and the Pacific railroad builders before 1900, agribusiness in California during the 1930s, and the struggle over water in California's Imperial Valley since World War II.

But the days of this empire are numbered, Worster warns. By 1985 the disastrous results of this imperial domination over nature in the West had become evident. These included social and economic stratification, exploitation of migrant workers, increasing pollution from pesticides and other chemicals, and the rise of an oppressive technocratic bureaucracy that threatened to undermine democratic processes. What is the remedy? As an enemy of bigness, Worster advocates accommodation to the environment, not its destruction. An admirer of E. L. Schumacher, author of *Small Is Beautiful* (1973), Worster speculates that the West would be a happier place, a cradle for freedom and democracy, if it were decentralized and controlled by millions of small farmers adapting to its environment rather than by corporate and governmental bureaucracies seeking to subject it.

A brief analysis cannot fully explore the complexities of this thoughtful, well-written, and well-researched volume. Worster's vision evokes sympathy, but his suggested remedies are more difficult to embrace. Yet, who can fault an author who urges us to put faith in the goodness and rationality of humanity and in a new version of the American dream? This provocative book raises significant questions about the West and should be read not only by those interested in the region but by all who are concerned with the course of American civilization in the twentieth century.

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T. LINDSAY BAKER and BILLY R. HARRISON. *Adobe Walls: The History and Archaeology of the 1874 Trading Post*. Foreword by B. BYRON PRICE. College Station: Texas A&M University Press. 1986. Pp. xx, 413.

For Americanists not specializing in the history of the American West, Adobe Walls may be a meaningless name. For specialists in the history of the nineteenth-century Southwest, of the Plains Indians, or of the extermination of the buffalo, however, Adobe Walls strikes a fascinating note. At this windswept trading post in the Texas Panhandle, established to serve hide hunters, took place in 1874 one of the most hotly contested Indian fights in the history of the Southwest Plains.

After the battle the victorious defenders packed up and returned one hundred fifty miles northeast to Dodge City, Kansas. Indians then swooped down on the abandoned settlement and burned everything but the sod saloon. Three-quarters of a century of wind, rain, snow, drought, and sun, and the pilfering of amateur pothunters took their toll. The remote site, deep inside the boundaries of the Turkey Track Ranch, remained unattended and, for much of the period, ignored.

In 1975 the Panhandle-Plains Museum of Canyon, Texas, began a systematic archaeological investigation of the site. Over a five-year period Billy R. Harrison conducted a total of fourteen months' investigation. This book consists of a summation of the work and T. Lindsay Baker's narrative of the history of Adobe Walls. It tells all one would ever want to know about the site and what happened there.

The book is divided into part 1, "History," and part 2, "Archaeology." Part 1 constitutes a thorough, in-depth history of Adobe Walls. The authors provide excellent background about the buffalo hide trade (as distinct from the robe trade). The characteristics of those unkempt frontiersmen, the hide hunters, are well described. The growing Indian unrest is explained. Finally, the attack is narrated, followed by the history of Adobe Walls to the present.

Part 2, "Archaeology," is equally thorough. It concerns another discipline, however, and may strike the historian as unusual. The methodology of another profession, the incredible thoroughness with which a dig is properly conducted, and the seeming worthlessness of the artifacts recovered may at first amuse the reader. An example might be "wear patterns" in the floor, indicating a heavily used doorway. Fifteen wild plum pits seem a bit insignificant, yet they tell something about the hunters' diet. And the identification of beer, gin, and above all bitters bottles, their manufacturer, and their dates of production are a bit exotic from the historian's perspective. Yet, as the authors point out in their conclusion, such evidence adds up to a picture of life styles on the Great Plains in the 1870s. I suggest assigning this book to students as a way of introducing them to the profession of archaeology and how it can enhance history. Many illustrations and a few maps and sketches accompany this worthwhile study.

RICHARD A. BARTLETT
Florida State University

DIANA KORZENIK. *Drawn to Art: A Nineteenth-Century American Dream*. Foreword by RUDOLF ARNHEIM. Hanover, N.H.: University Press of New England. 1985. Pp. xiv, 279. $35.00.

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In 1981 Diana Korzenik discovered a cache of papers that related the intimate history of a rural New Hampshire family from the mid-nineteenth to the twentieth century. By placing the family's history in the context of cultural and economic developments in the Northeast, Korzenik was able to reconstruct in unusually specific detail the educational and economic opportunities in the arts available to members of agricultural and industrial classes during the years surrounding the Centennial of 1876.

Korzenik's tale involves the family of Joseph Cross, a farmer from the region around Manchester, and his children—Joel Foster, Emma, and Henry—who were encouraged to study drawing by a prevailing enthusiasm for art and its promise of personal and national improvement. Prompted by manufacturers and art educators, school systems introduced programs designed to develop technical skills of drafting and drawing. Under these influences, and as a result of the friendship of a local artist, Henry Walker Herrick (1824–1906), who had succeeded in New York as an engraver on wood, the Cross children dedicated themselves to a life in the arts. Henry and Joel Foster obtained employment in Boston as wood engravers. Henry also developed an interest in landscape painting in a not-too-successful attempt to become a "fine" as well as commercial artist. When photomechanical blocks replaced wood engraving at the end of the century, the Cross brothers were unable to meet the lower costs and quick production of the photographic process; they presumably abandoned their artistic interests and faith in the value of art. Korzenik concludes that, with the frustration of art's promise, Americans "knew that artists were, or had been, important, but had forgotten why" (p. 255).

This is a naive conclusion. The Crosses experienced disappointments not because Americans had lost their respect for art but because this mid-nineteenth-century interest in industrial art—in contrast to the earlier interest in the fine arts—was more subject to the vicissitudes of changing technology and industrial demand. Actually, toward the end of the century, when the Cross brothers experienced their economic difficulties and subsequent loss of faith, interest in the fine arts was reaching a peak, but the distance between the fine and applied arts had considerably widened. Museums were abandoning their educational programs and concentrating their energies on aesthetic appreciation. Tastes were changing: Henry Cross's landscapes, which had never really succeeded, were out of fashion. The Cross family's experiences do not become a case study of what was happening in the American art world, but they are interesting as an indication of what was happening in the commercial or industrial world.

Nevertheless, Korzenik's book is a valuable contribution to American social and cultural history. Overlooking her simplifications and inelegant writing, the student of American material culture and society can find here a mine of information and suggestive insights about an important area in American national life.

LILLIAN B. MILLER
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EILEEN BORIS. *Art and Labor: Ruskin, Morris, and the Craftsman Ideal in America.* (American Civilization.) Philadelphia: Temple University Press. 1986. Pp. xviii, 261. $37.95.

The industrialism that altered the Western world in the late nineteenth century provoked many responses. The most familiar were the countervailing powers of labor, of governmental action against large enterprises, and of various kinds of utopianism. The arts-and-crafts movement, based on the thought of John Ruskin and William Morris, was among the most interesting of the utopian responses. Ruskin was a famous art critic but became a powerful critic of the competitiveness, waste, and lack of concern for individual workers in industrialism. He held that art was central both to individual and social life and could modify the excesses of industrialism. Morris tested these and his own ideas in workshops organized on noncapitalist principles, which produced goods unlike the standardized and mechanized fruits of industrialism. Both leaders and their followers sought to abolish divisions of class and labor and to make individual producers central to modern life.

The movement's aesthetics are well known and varied. Morris emphasized varieties of medievalism and designs based on natural forms. Others produced elegant pottery, glass, furniture, and housewares that were simple and showed the worker's hand. Mission-style furniture, probably the most famous of these forms, was popular among consumers seeking individual expression outside mainstream taste.

Women were prominent in the movement and gained some status, although they were unable to break the divisions of labor in the process of mass production. Eileen Boris concludes that they at least developed a new sense of sisterhood and of larger importance to society. In the end, the arts-and-crafts movement could not alter mass production and consumption because the system gave consumers what they wanted. The movement satisfied the longings and aspirations of many middle-class people for some kind of alternative to industrialism but remained marginal, however attractive many of its

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artifacts became. Ironically, it probably produced another strain of consumerism for its hand-made and often expensive products.

Boris's book is a welcome addition to the literature on the crafts movement, and she discusses its social as well as cultural importance. The best sections of the book deal with the adaptation of Ruskin's and Morris's ideas in the competitive American scene, with utopian colonies, and with the role of women. Her research is wide-ranging and her writing style elegant. She tells well an important story of the search for alternatives to the industrialism whose blessings were so mixed.

H. WAYNE MORGAN
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MAURY KLEIN. *The Life and Legend of Jay Gould.* Baltimore: Johns Hopkins University Press. 1986. Pp. xvi, 595. $27.50.

Attempts to rescue late nineteenth-century businessmen from hostile contemporary appraisals can be traced largely to the 1950s, when the idea of the Industrial Statesman seemed to supplant the notion of the Robber Baron. Turning to the most important industry for capital employment in the postbellum era, the railroads, scholars separated speculator-exploiters from system builders. They contrasted plundering promoters who amassed profits to satisfy private interest with constructive, imaginative business visionaries who contributed to the nation's development.

Maury Klein's study of Jay Gould, perhaps the most notorious of such figures, pivots on these same distinctions. Klein charges that it was the newspapers, misreading the business behavior of the era, "that forged the image of the robber barons," (p. xii) and created sources used by generations of writers to perpetuate simplified, distorted perceptions of the practices and personalities of the period. Gould, who became a legend in his lifetime of the archetypal speculator and monopolist, serves as a case in point. This scholarly biography, based on research in newly discovered sources, is designed to separate the man from the myth and finally to inter the villain of the contemporary press.

Business historians may challenge the contention that Gould was more inventive than his peers in developing new business devices, but they will be grateful to Klein for unraveling the complexities of confidential negotiations as he reinterprets Gould's career from the early days of Erie and the attempted Gold Corner through the heyday of developmental building.

Klein reveals a more complex, driven man than stereotype allows. Gould suffered the emotional turmoil of a childhood as the only male, and the youngest, of six children, whose mother died when he was four and who within the next four years witnessed the death of two stepmothers and a sister. Such biographical background helps explain the overprotective father who took refuge in the family, just as it adds dimension to contemporary caricatures of Gould's obsessive desire for control.

Nevertheless, Klein's intent to analyze Gould's personality to illuminate his career does not quite succeed. Rather, the author's constant intervention, his excessive sympathy for his subject, his repeated praise of Gould's daring, and his speculative, overly sophisticated explanations of Gould's motives, tend to backfire. In a biography so critical of the bias of previous works, this overbearing approach becomes all the more striking.

If Gould's methods were characteristic of the postbellum era, their application was different. Not merely New York high society and the press but also railroad promoters who relied on similar techniques found Gould objectionable. Indeed, this heavily documented account reveals the man as every inch the amoral, ruthless, grasping power seeker of popular criticism, a portrait reinforced by Klein's focus on Gould as the singular financial force and decision maker in building a vast business empire.

Klein's inquiry is so narrowly fixed on Gould that he fails to question the broader consequences of business behavior. For example, to revise the image of Gould as a speculator, Klein argues that his stock manipulations were a means to secure capital for other projects, not an end in themselves. But railroad leaders, as well as reputable investment bankers, similarly managed market operations to yield both quick profits and venture capital for railroads. The formulation begs the issue of whether construction of unnecessary lines created an unstable business environment of boom and severe depression that adversely affected the interests of other social groups and allowed insiders to exploit the fluctuations of the business cycle to profit from stock operations, receiverships, and reorganizations.

Then, too, the accounts of Gould's interregional rail projects, while attesting to his skill and concern for the condition of such properties, surprisingly omit a critical aspect of his career indicative of changing entrepreneurial activity in the corporate age. Gould, like other railroad leaders, recognized that individuals and groups of private capitalists could no longer meet the extraordinary capital demands for projected expansion and sought alliances with transatlantic private banking firms. Although Klein identifies J. P. Morgan as a director in two of Gould's companies by 1885, he neglects to examine the part played by other private bankers, such as J.&W. Seligman and Morton, Bliss and Company, who had shaped expansionary policy,

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provided share capital, and mapped financial plans for Gould's properties.

Finally, the portrait of Gould pales beside Klein's exposure of insider dealings and intricate manipulations, whose difficulties reveal a milieu deficient in institutionalized arrangements adequate to the new scale of business activity.

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H. L. POHLMAN. *Justice Oliver Wendell Holmes and Utilitarian Jurisprudence*. Cambridge: Harvard University Press. 1984. Pp. 223.

Centered on the legal philosophy of Oliver Wendell Holmes, Jr., this monograph measures Holmes mainly against the utilitarian ideas of Jeremy Bentham and John Austin, with brief references to Sir Henry S. Maine's historical jurisprudence and to late twentieth-century philosophers' search for superior moral norms. The main emphasis is on matching Holmes with Austin. Drawing in impressive detail on Holmes's writings and letters, H. L. Pohlman argues that Holmes's fact-oriented analysis of legal order has closer ties to Austin than some commentators have thought. As a corollary, Pohlman finds Holmes's contributions less distinctive than others have rated them. He presses the point further than carries full conviction. But anyone who shares the familiar fascination with Holmes's career will find this study a useful stimulus to more precise appraisal of the course of jurisprudence over the past century.

Central chapters deal with the earlier utilitarian philosophers and Holmes on themes of liability, sovereignty, and judicial law making. Another key essay finds the critical, unifying factor in the jurisprudence of Austin and Holmes to be their assumptions about the proper method of analyzing legal order. That method gave priority to the realistic, unprejudiced examination of the wants or perceived needs of people in society and of their experiences in pursuit of these wants and needs. This emphasis spelled an inductive approach to understanding how judges made policy through the cumulative effect of particular decisions. The author gives due credit to Holmes as legal historian, especially in his classic, *The Common Law* (1881), and persuasively refutes critics who—underestimating the extent of analysis of policy in that famous book—have claimed that Holmes sought to impose unreal logic on the course of judicial law making. But Pohlman perceptively finds Holmes's central interest to lie not in legal history but in legal philosophy; for Holmes the historian's primary job was to keep the philosopher's feet on the ground.

This study will be serviceable to historians of jurisprudence but will offer little help to students of the dynamics of policy making across the full range of legal agencies. This is not to criticize the quality of the monograph but only to mark limits inherent in its subject matter. Holmes focused on judicial process, viewed with reference to some relatively abstract concepts; he is not very helpful to historians who wish to assess problems of legal order posed by gross inequalities of power among interests competing in legal arenas and by the tendency for policy to be fragmented in a pluralist society.

JAMES WILLARD HURST
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ROGER LANE. *Roots of Violence in Black Philadelphia, 1860–1900*. Cambridge: Harvard University Press. 1986. Pp. 213. $25.00.

Recent Afro-American historiography has moved away from the model of victimization and social pathology to stress instead the strength of the black community and black culture in the face of adversity. Although this trend has led to major works of scholarship, it has at times obscured the impact of white oppression on black life. This book, as the author acknowledges, "returns to the older tradition" to confront an issue largely ignored by the new scholarship—black criminal violence (p. 3).

Despite his local focus, Roger Lane has produced an ambitious and important work. Lane argues that in the mid-nineteenth century urban violence in general began to decrease—a trend that continued until about 1960. This decrease was the result of the more orderly and predictable quality of urban life that accompanied industrial growth, expanded economic opportunity, and the emergence of a complex institutional structure. Blacks, however, were shut out of the new economic system; in contrast to the order and predictability of white urban life, blacks experienced "a world of varying degrees of discrimination, full of unpleasant surprises and dead ends, which had important consequences for social behavior" (p. 16). Whereas whites socialized their aggression and avoided the kind of behavior that led to violence, blacks were "uniquely cut off from the experience of the urban industrial revolution . . . [and] did not share in the process which imposed a new social psychology, and thus lower crime rates" (p. 140).

Lane supports his thesis with a painstaking examination and often ingenious use of evidence from late nineteenth-century Philadelphia. A specialist in the history of crime and violence, Lane uses criminal records to better effect than have previous scholars of black urban America. He also uses manuscript census reports and newspapers in unusually re-

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sourceful ways. The result is the fullest portrayal thus far of a black urban underclass. Lane demonstrates that blacks, shut out of legitimate economic and political enterprises, created an elaborate criminal subculture based on petty theft, political payoffs, gambling, and, most importantly, prostitution. This subculture, "rich in the potential for violence," made blacks "far more likely than whites . . . to retain an older or preindustrial social psychology in which aggression was primarily directed outward, at others" (p. 143).

Middle-class blacks were the direct and indirect victims of black crime and violence. They were often robbed and beaten by black criminals, and their efforts to achieve stability and respectability were thwarted by the proximity of the black underworld. Moreover, whites used black criminal behavior to justify prejudice and discrimination against all blacks. Continually on the defensive, middle-class black leaders blamed black crime on the heritage of slavery and the traumas of migration. They could not face the bleak truth that black crime and violence were increasing and were the product not of problems imported from the rural South but of social conditions in what had been hailed as the promised land.

This well-conceived, solidly researched study has far-reaching, and depressing, implications. It reminds us that, despite the resilience and resourcefulness of the black community, the cost of racial discrimination and oppression has been enormous.

ALLAN H. SPEAR
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LAWRENCE H. LARSEN. *The Rise of the Urban South.* Lexington: University Press of Kentucky. 1985. Pp. xi, 220. $22.00.

Like all other U.S. urban historians, Lawrence H. Larsen must have learned early that the federal government had created a remarkably detailed "snapshot" of American cities as a result of the "centennial" tenth census of 1880. The reports of that census, including George E. Waring's *Social Statistics of Cities*, are, moreover, supplemented by some useful sets of data (for example, *Statistics of Churches*) from the tabulation of 1890. Unlike the rest of us, however, Larsen has decided to mine this rich lode. He envisages four regional volumes on American cities in the last years of the nineteenth century—one each on the West, the South, the Midwest, and the Northeast. This book is the second of these studies to appear.

Larsen has produced a modestly sized volume (a little over one hundred sixty pages of text and just under fifty pages of documenation) consisting of seven topically organized chapters. The introductory chapter addresses post–Civil War southern views of cities and contains many observations by "New South" publicists. A chapter follows on the population of southern cities in 1880, arranged by size and by geographic "layer" (that is, Atlantic Coast, fall line, New West, Gulf Coast, and inland transportation centers)—a classification scheme that does not contribute significantly to a better understanding of urbanization or of the urban experience. A third chapter analyzes population elements (such as race, sex, age, nativity), manufacturing, religious organizations, and educational institutions in southern cities from 1880 to 1890 and makes some gross comparisons to national norms. This section leans heavily on statements by Edward King and Henry W. Grady. The next chapter addresses transportation patterns and their impact on urban growth and is followed by another divided into two roughly equal segments, one dealing with city government and finance and the other consisting of a mélange of comments on segregation, housing, physical appearance, parks, plantings, and architecture. The sixth is a valiant effort to reduce to some manageable scope an examination of what might be called the housekeeping functions of the cities—including such diverse elements as street surfacing, waste disposal, gas supply, public health and life expectancy, and crime and policing. Finally, a heavily quantified afterword sketches the trends in southern urban development in the decades following 1880. Throughout this work Larsen introduces a limited amount of comparison to national norms and nonsouthern experiences.

Beginning with the descriptions of 1880 as a final product, Larsen asks how the southern cities had developed. He seeks the answers to his questions in a large number of general works and not a few largely local and frequently obscure sources—newspapers, theses and dissertations, articles from state journals, and local monographs. The added dimension resulting from this industrious search constitutes one of the more significant strengths of the study.

This volume exhibits few real weaknesses, but Larsen is less successful in some areas than in others. He probably allows his conceptualization to be too heavily influenced by the rhetoric of the New South. Although he sometimes notes that little was new about the call for urbanization and industrialization in the postwar South, the tone of his writing too often suggests that these late nineteenth-century aspirations and developments were novel to the region's tradition. Also, despite an occasional comment about urban conditions in other sections or at other times, the book lacks the rigorous comparative structure (both intraregionally and extraregionally) that Larsen's superior data deserve.

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With this book Larsen reaches the midpoint of his proposed national survey of urban life in the United States at the end of the nineteenth century. This is a worthy goal, well executed, and the product is useful in a way that differs from either the broader perspective of a general survey (such as that by Blake McKelvey) or the more narrowly focused view provided by the intensive studies of individual cities. Urban historians have reason to be grateful.

LEONARD P. CURRY
University of Louisville

JOHN BODNAR. *The Transplanted: A History of Immigrants in Urban America.* (Interdisciplinary Studies in History.) Bloomington: Indiana University Press. 1985. Pp. xxi, 294. $27.50.

History abhors a metaphor. Consider all the talk and ink sucked into the vacuous debate over whether America is better likened to melting pot, salad bowl, mosaic, or whatnot. Fortunately, John Bodnar's synthesis of immigrants' life in urban America from 1820 to 1940 does not stop with labeling them "transplanted" rather than Oscar Handlin's "uprooted." Literally the immigrants were both, or neither; the human story is too complex for horticultural analogies. Still, one might propose "the engrafted" (if it were not already appropriated by politics), since immigrant culture was, as Bodnar concludes, "not based exclusively on ethnicity, tradition, class, or progress. More precisely, it was a mediating culture which confronted all these factors. It was simultaneously turbulent and comforting. It looked forward and backward, although not very far in either direction" (p. 212). Herein Bodnar far transcends recent efforts to escape Handlin's influence. Instead, *The Transplanted* brings to fruition, so to speak, ideas that were implicit in *The Uprooted*—and before that in Marcus Hansen's pioneering work in the European sources.

Where Hansen led, indeed, Bodnar is most satisfying. Emigrants, for all their regional peculiarities, generally came from the "middle and lower-middle levels" (p. 13) of their homelands: middling landholders and artisans early on, later increasing numbers of rural and industrial laborers. On the one hand, emigration was a means, for those who could afford it, to sustain the traditional family against the disruptions of the modern world. On the other, the immigrant family itself had to modernize. In either respect, immigrants were "children of capitalism" (p. 1). No matter that industrialization separated household from workplace; since kinship still constituted the best employment agency, it was the "stable core" (p. 71) of the ethnic clusters (not ghettos) in American cities. The family found jobs for members and friends; wages sustained the family. It thus served as the mediating institution *par excellence* in the new urban economy as it had in the villages of the Old World. Thus far Bodnar agrees with those who adjudge the modern family to have been no less effective for its purposes than the traditional one.

The modern workplace, however, was outside the home, and if wages were too low and hours too long the worker needed "more sophisticated forms of organization" (p. 85): craft unions, industrial unions, socialism, the Industrial Workers of the World. Bodnar does not draw as sharp a line as other historians between the old-fashioned objectives of most immigrants and the proletarian radicalism that never succeeded in modernizing them as it did the less self-sufficient laborers who remained in the old country. He does set working-class conservatism squarely against the upward-striving individualism of some leaders of ethnic groups. Other historians also have argued that mutual-aid societies, like emigration itself, began in Europe as a middle-level alternative to proletarianization, but in America, Bodnar concludes, their membership was more working-class (even when arrayed in gentry costumes) and their goals more political. The leaders, however, remained close to "the business culture of urban America" (p. 118). He stops short of considering whether the American "mainstream" (p. 184) may also have been more concerned with the security of the modestly self-reliant family than with the extraordinary opportunities presented by capitalism.

In his chapter on immigrant churches Bodnar similarly emphasizes schisms between leaders and followers more than the near-universality of the arena that religion provided. Bodnar also accepts the ethno-religious thesis of the 1960s and 1970s on politics, which is now under attack. Urban machine politics, as an extension of the economy of family and neighborhood, had little to do with workplace issues, which were left to other sets of leaders. Finally, Bodnar's discussion of ethnic "folklife" is limited by the paucity of monographs; American historians (though not he) still disdain the symbolic culture of lodge halls, festivals, parades, and picnics. We have little taste for cheese and none at all for worms—or peasants.

In this first coherent synthesis since Handlin's, Bodnar establishes (though with small help from his copyeditors and proofreaders) that urban immigrant life was neither traumatized by modern capitalism nor fixed in primordial tradition but, rather, transformed by the dynamic between these forces. Whether in this the immigrants were not, after all, more like than unlike their old-stock American contemporaries remains to be explored.

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GERALD SORIN. *The Prophetic Minority: American Jewish Immigrant Radicals, 1880–1920.* (Modern Jewish Experience.) Bloomington: Indiana University Press. 1985. Pp. x, 211. $24.95.

STEPHEN J. WHITFIELD. *Voices of Jacob, Hands of Esau: Jews in American Life and Thought.* Hamden, Conn.: Archon. 1984. Pp. 322.

Voices of Jacob, Hands of Esau is a beautifully crafted, intelligent, insightful, and often witty collection of essays by Stephen J. Whitfield, a gifted interpreter of American Jewish culture and letters. Although these essays, for the most part, are versions of previously published articles and chapters, their collection in one volume is more than just a convenience. Whitfield has provided substantial cultural evidence for a conclusion that many have arrived at intuitively, namely, that the American Jewish scene is characterized by the persistence of the critical "voice of Jacob," the insightful and often astringent perspective of the outsider, even as Jews have acculturated—have been attracted by the temporal power of the "hands of Esau." Whitfield notes that, even with rapid secularization, it is striking "how tenacious many Jews have been in keeping at least the husk of faith and the sense of peoplehood" (p. 1). Indeed, the degree of assimilation may be inversely related to the commitment to ethnic and cultural distinctiveness.

Whitfield demonstrates that the tension between the forces of acculturation and persistence generates much of the cultural and intellectual creativity and vitality of American Jews, often informed by the perspective of the outsider, in several areas: liberal and radical politics; the historical awareness of the issues of totalitarianism and anti-Semitism; contributions to American mass culture, particularly in comedy and music; contributions to journalism, a profession that requires a critical intelligence; and, by contrast, Jewish accomodation to the South, where racism and cultural homogeneity have heightened the tension between acculturation and particularism.

Broad, far-ranging, and incredibly entertaining and edifying, Whitfield's work has furthered our understanding of the impact of American Jews on central areas of American life and thought and, in turn, the influence of American culture on them.

Less satisfying is Gerald Sorin's *The Prophetic Minority*. Like Whitfield he is intrigued by the particular affinity that many Eastern European Jewish immigrants had for socialism, labor unionism, and radical politics. Both authors share the belief that this phenomenon is partially the result of history, of proletarianization, of experience, of sensibility and outlook, and of generational conflict. They part ways, however, in emphasis. For Sorin these "structural" factors are certainly important, but they are secondary to the pervasive influence of "Yiddishkeit" and Jewish ethical, religious, and messianic impulses and traditions. Using interviews and documentary material from 170 American Jewish socialists, Sorin concludes that the imperatives of social justice that characterize the Jewish tradition permeated and influenced these immigrants and made them more receptive to radical ideologies and humanistic concerns.

Although Jews undoubtedly have been disproportionately represented among the Left in the United States, few Jews have been radicals. Ever since Karl Marx, who was born a Jew, baptized a Lutheran, and nurtured without the benefit of any Judaic learning, some Jews have been conservative, many have been apolitical, and most have been liberal, not radical. Radical Jews were probably not as influenced by the Jewish messianic tradition of "Tikun Olam" (repair of the world) as Sorin would have us believe. How does his theory account for the majority of Jews, who were probably not so influenced? How can his hypothesis be tested? In quotations cited by Sorin, this cultural heritage appears to have been transmitted to often unbelieving Jewish socialists through an unconscious process. This may be true, but it is extremely difficult to verify. Immigrants who were most knowledgeable about Judaism, who were most conversant with the prophetic traditions and texts, were, in fact, the least likely to be sympathetic to socialism. The probable reasons for this are the important differences between Jewish and socialist messianism, differences not as easily bridged as Sorin suggests. Jewish messianism was closely tied to the concept of the Jews as a chosen people, to the particularities of Judaism, and to belief in a messiah. Even in its secular forms, messianism was more aligned to Zionism than to socialism. Socialist messianism, in contrast, was universalistic and rejected nationalism and, in most cases, Judaism because of its particularistic elements.

This does not mean that certain traditions of Judaism were not related to the Jewish contribution to radical movements in America. Clearly a relationship existed, and undoubtedly many Jewish socialists were influenced and informed, maybe even subconsciously, by these tendencies. To draw as broad a conclusion as Sorin does, however, based on the type and extent of the evidence he adduces, does not seem reasonable.

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Hobart and William Smith Colleges

ALIXA NAFF. *Becoming American: The Early Arab Immigrant Experience.* (Middle East Research Institute Special Studies.) Carbondale: Southern Illinois University Press. 1985. Pp. x, 376. $19.95.

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DAVID M. REIMERS. *Still the Golden Door: The Third World Comes to America.* New York: Columbia University Press. 1985. Pp. xviii, 319. $25.00.

These excellent volumes exemplify the continuing vitality of historical scholarship on immigration and ethnic life. David M. Reimers's book is a comprehensive account of Third World immigration to the United States from World War II to the present. Alixa Naff's book examines the experience of one group, immigrants from greater Syria, in the late nineteenth and early twentieth centuries. Both are scholarly and well written and deserve a wide readership.

Reimers asks the classic questions: Who came to the United States? Why? Where did they settle? What work did they do? How did America react to them? He answers thoroughly and intelligently, using congressional records and reports, documents of the Immigration and Naturalization Service, newspapers and magazines, and the extensive scholarly literature on recent immigration.

Reimers notes differences between Third World immigrants, now the majority of newcomers, and their European predecessors; Third World immigrants are more likely to be women, refugees, or "illegals." He notes new "push" and "pull" factors: industrial as well as agricultural poverty, a lack of satisfactory employment for educated elites in Third World countries, "revolutions" in transportation and communication, and the increased importance of overseas American military personnel, businesses, and media in disseminating information about life in the United States. He also notes the new role of voluntary agencies and the federal government in the resettlement of refugees.

Reimers provides anecdotal descriptions of the immigrant experience—Cuban exiles becoming bank presidents, Hmong tribesmen trapping dogs in Golden Gate Park—but his most valuable contribution is his exploration of policy issues. He notes that the Immigration Act of 1965 abolished past discrimination but failed to control future immigration because its framers did not anticipate that Asians would use the family reunification provision to bring in brothers and sisters as well as parents and children and thus expand the numbers of nonquota immigrants geometrically; that refugees admitted in excess of legislated quotas by the president would become an important source of immigration; and that the end of the *bracero* (temporary worker) program with Mexico and increased social and political turmoil in Central America and elsewhere would cause unprecedented illegal immigration.

Reimers's analysis of recent efforts to establish a new immigration policy acceptable to conflicting political, economic, social, and moral interests is masterful, as is his analysis of controversies about the number and economic impact of the undocumented aliens and the definition of "refugee" and "asylum" status. He considers the Reagan administration's policy of admitting Russian and Indochinese but not Haitian and Salvadoran refugees "disgraceful and unfair" (p. 251). His conclusion—well supported by his data—is that current immigration policy "favors the highly educated, skilled, and those with connections and influential constituencies in the United States" and will probably continue to do so (p. 253).

Like Reimers, Naff has a talent for explaining complexities. Her first chapter, "Land, History, and Society," provides a succinct introduction to the history and culture of what would now include Syria and Lebanon. This introduction describes the importance of family, village, and religion in establishing personal and communal identity and the cultural salience of individual and family pride, honor, and loyalty to leaders.

Naff builds on this base to explain patterns of ethnic life and assimilation in the United States. She notes the early religious divisions of Arabs in America and the importance of family and village loyalties within these divisions. Newspapers, clubs, and charitable societies proliferated but were often parochial and short-lived because "factional and clannish tendencies, combined with an ingrained individualism, inhibited the Syrian's ability to sustain, enthusiastically, anonymous support for causes or participation in organizations or institutions involving nonprimary groups" (p. 308). Syrian immigrants' traditional competitiveness and consciousness of status stimulated them to acquire American manners and possessions as barometers of success; thus, ironically, traditional patterns of behavior facilitated Americanization.

Naff's main sources are interviews with first- and second-generation immigrants. The book is rich in material on family and community life, but the primary focus is on peddling as the almost universal experience of Syrian immigrants—an atypical experience in an age of urbanization and industrialization. The book includes memorable accounts of the hardships—and occasional pleasures—of trudging through a strange country in all kinds of weather to sell pins, trinkets, linens, and rugs. One woman recalled that as a child she threw sand in a policeman's eyes to avoid arrest for peddling without a license. Another remembered her impatience at the age of nine when two well-meaning customers gave her lunch and a new dress: "I didn't want to be held up. I wanted to sell" (pp. 175–76). The importance of women's economic roles as peddlers and later as shopkeepers is emphasized. Women had better access to American homes than did men, were trusted by female customers, and initiated

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profitable new kinds of merchandise, such as bed linens and women's clothes.

Naff's book is one of a number of recent studies showing the importance of ethnic and family networks and traditional culture in promoting rather than inhibiting Americanization and social mobility. It is also a significant contribution to the literature on ethnic enterprise and on immigrant women. Finally, it provides valuable insights into the assimilation of a relatively small but important non-European group that has been insufficiently studied heretofore.

MAXINE S. SELLER
State University of New York, Buffalo

ALEXANDER KEYSSAR. *Out of Work: The First Century of Unemployment in Massachusetts.* (Interdisciplinary Perspectives on Modern History.) New York: Cambridge University Press. 1986. Pp. xviii, 469. Cloth $49.50, paper $14.95.

Alexander Keyssar has written an important and detailed study of the experience of unemployment in Massachusetts. Using a variety of historical methods and varied historical sources, Keyssar argues that unemployment was a pervasive working-class experience and not just a problem of business downturns or adjustments in the labor market. Keyssar finds that between 1870 and 1920 roughly 20 percent of the labor force experienced idleness at one point or another during the year. Moreover, during depressions both the number out of work and the time without work increased. This unemployment put severe strains on the resources of the working class. It touched both skilled and unskilled workers, immigrants and native born, men and women. The pervasiveness of unemployment and its accompanying insecurity structured the working-class experience and helps explain the nature and form of its institutions. Keyssar finds that the ever-present fear of unemployment partially explains the high degree of working-class mobility and the tactics of American trade unions.

Keyssar argues that, although joblessness and poverty existed before industrialization, the rise of industrial wage labor—an urbanized proletariat, dependent on wages for subsistence—linked joblessness and poverty and created modern unemployment. Using a variety of sources, he demonstrates widespread unemployment in both large and small cities. He also shows its persistence throughout the late nineteenth and early twentieth centuries, in both good times and bad, and its impact on workers and their families.

In the second half of the book Keyssar shifts from the experience of unemployment to the response to it. Beginning at the bottom and moving up, Keyssar looks first at workers' attempts to cope with the crisis of job loss, the response of the union movement to the problem, and finally society's response. He argues that, although workers' responses tended to remain fairly constant from 1870 to 1920, in that same half century society ceased to see the problem as one of personal failure or temporary adjustment, requiring no public intervention, and began to see it as a social failure requiring some sort of social response—private (business's answer) or public (the progressive's answer).

This work has many strengths and few weaknesses. Three particular weaknesses bothered me. Keyssar mentions multiple reserve labor pools. How and why those pools remained separate is an important question that Keyssar should address. He discusses the difference between unemployment for seasonal workers and unemployment for the non-seasonal but then tends to merge the two experiences. Finally, the second half is much more readable than the first.

JOHN T. CUMBLER
University of Louisville

RAYMOND A. MOHL and NEIL BETTEN. *Steel City: Urban and Ethnic Patterns in Gary, Indiana, 1906–1950.* New York: Holmes and Meier. 1986. Pp. x, 227. Cloth $32.50, paper $23.50.

Gary, Indiana, is a city forged out of the Industrial Revolution. Cut off from Lake Michigan by the huge steel mills of its creator, U.S. Steel, the city is representative of medium-sized American industrial cities. Its demographic history offers proof of the vast transformation brought on by industrialization. Raymond A. Mohl and Neil Betten have attempted to discuss the vast population changes in Gary from its earliest years through 1950. They have done a fine job examining the white ethnic, black, and Hispanic communities and the often unfriendly reception these groups received in the "Steel City."

Mohl and Betten have based their book on extensive research in the available local, state, and national archives. Unfortunately, U.S. Steel did not permit access to its records. Nevertheless, Mohl and Betten have looked at a wealth of material and produced a book that traces Gary's rise from company town to multiethnic, multiracial city. The authors also constantly remind the reader of the larger story of industrial America.

The book is an interesting description of the American industrial city as small town. Gary appears in many ways—at least in its white establishment—to reflect small-town Midwestern culture. Despite its

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birth out of a national steel merger, Gary remained somewhat parochial in viewpoint.

For Mohl and Betten Gary's failure seems to have arisen from its success, which is the point of Gary's history and of much of urban America's. The city was created not as a haven for immigrants, blacks, Hispanics, or even Anglo-Saxon Protestants but as a money-making venture. Gary existed because U.S. Steel wanted a good location to produce steel where it could control the political, economic, and social climate. Out of the desolate track of sand dunes rose a "magic" city, but magic for whom? The answer was obvious—for the investors of Judge Elbert Henry Gary's concern. For those drawn to Gary in search of work, the city did not always seem quite the success it did to U.S. Steel's stockholders.

Working-class Gary suffered from the problems of poor housing, ill-health, poverty, and racism common to other urban industrial places. U.S. Steel's urban planners did not even bother to plan for the army of semiskilled and unskilled workers who operated the mills. A shantytown and an industrial slum resulted. So did the steel strike of 1919, which was handled with ruthless efficiency by U.S. Steel aided by federal troops under the command of would-be presidential candidate General Leonard Wood.

Mohl and Betten describe working-class Gary with obvious sympathy. The authors have tried to give the point of view of Gary's workers toward several issues. Their discussion of Americanization and the public school is especially enlightening.

This book, however, is not without its flaws. Some of the writing is repetitive. The authors' tendency to constantly drive home a point can be at times irritating. This problem is minor in an otherwise solid study of a smaller American industrial city and its diverse population. The book provides valuable insights for those interested in American urban, labor, and ethnic history.

DOMINIC A. PACYGA
Columbia College

KATHY PEISS. *Cheap Amusements: Working Women and Leisure in Turn-of-the-Century New York*. Philadelphia: Temple University Press. 1986. Pp. xi, 244. $24.95.

Post-Vietnam American historiography has at last awakened to the importance of leisure activities in the cultural and social history of the United States. Books on sports, amusement parks, movies, and other forms of pleasure have proliferated during this decade. Urban historians recognize that, to be complete, a city's history must include an analysis of recreational outlets available to all its inhabitants. All too often, captivated by their subjects, cultural historians have centered their studies on the more affluent and noticeable members of society; the Belmont Race Track was far more interesting than a family excursion to Gravesend or Coney Island. The emphasis is changing, however, as younger historians rediscover their roots and expose the "underside" of American urban life.

In her well-researched and scholarly exploration of working women in turn-of-the-century New York, Kathy Peiss has joined the ranks of American scholars writing history from the "bottom up." Examining this seminal period in New York's long and colorful history, Peiss shows working women flocking to dance halls, amusement parks, clubs, and nickelodeons, seeking pleasure, escape, and autonomy and experimenting with new sexual and social relationships with men.

We learn that although working-class families did not engage in any substantial recreation during this period working-class women did. For them recreation represented a break with the restraints of immigrant and postimmigrant life. For many of these women recreation was part of the process of Americanization; for others, it was another important step toward independence. Women were assisted in this evolution by a decline in working hours and an increase in job opportunities, which reinforced the appeal of recreation in the public sphere and strengthened women's notions of leisure through positive interactions within the work place. For New York's working women leisure was a way of expressing individuality and independence. It was a liberating experience.

Young city women used the streets, social clubs, dance halls, and theaters to carry on cultural styles all their own, and rising cultural entrepreneurs supplied the outlets to feed and cultivate those styles. Capitalizing on the needs of salesgirls, factory workers, and parlor maids, "these commercial amusements transmitted and mediated heterosocial culture to working class women and men, although in different ways and to different audiences" (p. 187). This same commercial culture, argues Peiss, led women to tie self-fulfillment to consumerism and discouraged a feminist consciousness.

Citing Coney Island as the symbol of this transition, the author concedes that it "exemplified . . . not only the decline of genteel middle class cultural hegemony . . . but the rise of a heterosocial culture that owed its form in part to that structure of working class social life" (p. 137). After 1900 movies replaced Coney Island in popularity and marked a decisive break in the pattern of working-class amusements. More importantly, Peiss proposes that movies altered women's participation in public, commercial amusements.

Peiss sees this working-class movement as a reflection of the liberation of women, a cultural process in which leisure and working women played a substan-

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tial role: "This desire of women for self-determined pleasure, sexuality, and autonomy, haltingly expressed by working women at the turn of the century, continues to be a compelling issue several generations later" (p. 188). But Peiss is not convincing here and leaves the reader wondering whether "girls just wanta have fun" is a trivial, musical claim or a profoundly liberating, though unfulfilled, feminist demand. The book is vague on this issue.

A valuable study in urban, women's, social, and cultural history, Peiss's book fills a gap in our study of contemporary American life. Unfortunately, the author manages to drain much of the color and excitement from a topic and period rife with drama and pageantry. Painting her portrait white instead of mauve, Peiss tells her tale in a methodical, humorless, and deliberate style, compiles data in a rapid-fire fashion, and overwhelms the reader with a morass of information that is repeated in different forms throughout the book. It is not something a reader peruses eagerly. In spite of these artistic shortcomings, this book deserves wide distribution and reading among a serious and caring historical community.

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LAURA SHAPIRO. *Perfection Salad: Women and Cooking at the Turn of the Century.* New York: Farrar, Straus, and Giroux. 1986. Pp. 280. $16.95.

This volume belongs to the "advice to women" genre of historical research. Laura Shapiro focuses on that part of the domestic science movement concerned with cooking. It is a fascinating story that covers the burgeoning interest in food at the turn of the century, its ideological underpinnings, the founding of cooking schools that sought to educate a diverse group of women on proper methods of cooking and menu planning, and the movement's influence in molding a distinctly American cuisine.

Instruction in scientific cookery was not limited to middle-class women in the East. Armed with the conviction that wholesome food made for wholesome lives, the leaders of the movement sought to instill its principles in urban slums, prisons, reformatories, and on Indian reservations. Shapiro briefly summarizes the domestic science movement and analyzes it in familiar terms as a response to the "woman question." By professionalizing the domestic sphere, leaders of the movement hoped to enlarge and elevate the role of women, which had been greatly attenuated by industrialization. This is not, however, the book's contribution; domestic science and its outgrowth, home economics, have been analyzed in far greater depth elsewhere.

Shapiro is at her best when she describes the various elements of scientific cooking. Foremost among them was an interest in the techniques of cooking and menu planning rather than eating. The leaders of the movement, which included the founders of the major cooking schools, showed a complete disregard for the flavor of food; their concerns were its constituent parts, digestibility, and appearance. The "gratification of appetite" was unladylike and unscientific (p. 72).

At a time when the science of nutrition was in its infancy, the cooking schools encouraged their students to serve massive amounts of carbohydrates, fats, and proteins. Since vitamins were unknown, fruits and vegetables were looked at askance. If served at all, they had to be tamed. A banana-nut salad was confined in a banana peel, and gelatin, which bound a variety of ingredients together, made for a dainty salad. This homogenization was part of the emphasis on control and precision; experimentation was strongly discouraged in scientific cookery.

Because Boston was the center of the movement, many traditional New England dishes—baked beans, codfish balls, Johnny cake—came highly recommended by the scientific cooks. Herbs and spices, as well as foreign cuisines, were suspect; blandness of taste was a major goal. The most salient symbols of the movement were white sauce and white meals. All manner of food—potatoes, vegetables, meats—were smothered in a simple, untempered sauce of milk, butter, and flour. Meals consisting entirely of "white food"—boiled cod, mashed potatoes, rice, and macaroni pudding was a typical menu—were widely touted.

The aim of scientific cookery was the Americanization of food and its reduction to a bland common denominator. Readers will not be surprised to learn that such characteristic American culinary items as Jello salad, American cheese, and catsup all had their origins in the movement.

Shapiro's book is a lively, well-written, and often amusing volume. Although at times short on analysis, it contributes a wealth of data on a topic that has been too long ignored.

MAXINE L. MARGOLIS
University of Florida

PATRICIA R. HILL. *The World Their Household: The American Woman's Foreign Mission Movement and Cultural Transformation, 1870–1920.* (Women and Culture.) Ann Arbor: University of Michigan Press. 1985. Pp. 231. $19.50.

The interdenominational woman's foreign mission movement appealed to the domestic piety, evangelical sisterhood, and intense personalism that characterized the Victorian woman's sphere; it was the

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largest woman's movement in the nineteenth century. Patricia R. Hill's important study describes the rise and fall of this extraordinary crusade that by 1915 enrolled more than three million women in female missionary societies of some forty denominations.

Women attracted by the romance of missions found congenial objects in the women and children of heathendom, who were largely off-limits to male missionaries. Following the Civil War, their prayerful support sent several thousand women, largely single, to the field to address the needs of a population previously tended only by overworked missionary wives and mothers. The limited autonomy of the women's boards, which were invariably affiliated with a general board under male direction, allowed for the development of a separate female mission and the expansion of the gender horizons of conservative churchwomen.

Hill is perhaps most original in charting the precipitous decline of the female missionary movement, which accompanied the wane of the woman's sphere of prayer and feeling. Professionalization contributed to the declining enthusiasm of the rank and file. The missionary enterprise initially rested on thousands of letters exchanged directly between local societies and missionaries or converts in the field. But the boards gradually came to frown on such sticky personalism and encouraged centralized and systematic support through headquarters. The boards' new pride in bureaucracy was embodied by publicity photographs of empty offices—no feminine touches, no people, and hence no evidence that these offices belonged to women.

By losing their special sense of womanly mission, the women's boards lost their reason for separate being. Female missionary bureaucrats fought valiantly to hold onto their institutional autonomy, but their supporters, left behind by "the science of missions," could marshal little enthusiasm. When the parent boards absorbed the once-powerful women's boards in the 1920s, women's special interests were represented only briefly (until the retirement of women officers) on the new combined boards.

The fate of the separate women's boards was also influenced by the improving global fortunes of women. Initially American women offered their Christianity as the best hope for the women of Asia and Africa who were subjected to such sensational "heathen" practices as footbinding, child marriage, and suttee. In the twentieth century the support of women's boards for Christian home life ironically set them at odds with religious liberals whose efforts to tolerate host cultures could allow for the continued maltreatment of women. Debates within the mission movement largely aside, though, the most dramatic abuses of women overseas diminished. As the bars between the sexes fell, missionary men could proselytize among women, and American women lost their unique mandate.

The nearly total demise of the women's foreign mission movement is perhaps best indicated by its only recent rediscovery by historians. Hill's study beautifully restores the centrality of this crusade to both the flowering and the fading of nineteenth-century women's culture.

JANE HUNTER
Colby College

JUDY BARRETT LITOFF. *The American Widwife Debate: A Sourcebook on Its Modern Origins.* (Contributions in Medical Studies, number 18.) New York: Greenwood. 1986. Pp. xii, 251. $35.00.

Judy Barrett Litoff has written a useful documentary history of the early twentieth-century controversy concerning the appropriate primary caregiver for childbearing women. Litoff, an expert in the area of American midwifery, includes primary documents that chronicle the rediscovery of the midwife and the dialogue among those who opposed midwifery, supported it, or were willing to accomodate it under certain conditions. Litoff places these documents in the social and cultural context of their times. The references and bibliographical essay together give direction for future research on the history of American childbirth.

Potentially the most significant part of the book is Litoff's abbreviated comparison of the midwife debate of 1910–30 to the continuing problems of midwifery today. If history provides lessons for the present and direction for the future, the book would have served a greater purpose by juxtaposing contemporary views on midwifery with the earlier debate to demonstrate the continuing impact of medical ideology and the greater politicization of the current midwife debate. Litoff outlines several ways in which the early twentieth-century midwife debate was similar to and different from the current debate, including issues related to the continuing medical demand for dominance in maternity, the ambiguous legal standing of midwifery, infant and maternal mortality and morbidity, and midwifery as a component of women's rights. Litoff is to be commended for the choices she made in this book, but her valuable insights concerning the continuity of the midwife debate beg for the selection and interpretation of documents that graphically demonstrate why the early American midwife debate is the source of the modern debate. The book is

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certainly recommended for what it is but kindly judged for what it might have been.

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BAILA ROUND SHARGEL. *Practical Dreamer: Israel Friedlaender and the Shaping of American Judaism.* (Moreshet Series, Studies in Jewish History, Literature, and Thought, number 10.) New York: Jewish Theological Seminary of America; distributed by KTAV, Hoboken, N.J. 1985. Pp. xxvi, 223. $20.00.

In 1920 Israel Friedlaender was brutally assassinated while on a mission of mercy to the Ukraine in behalf of its beleagured Jewry. At the time of his death, Friedlaender, chief lieutenant to Solomon Schechter and an important figure in the burgeoning Jewish Theological Seminary, was somewhat less than a household name to most American Jews. But his was a most significant voice for the indigenous German Jewish elite and the emerging Americanized East European leadership who would come together not only at the seminary but also within the New York Kehillah. Friedlaender spoke of the necessity for Jews to accept the challenges of modernity and secularism even as he maintained an abiding allegiance to the ceremonial teachings of the heritage. He was a prime advocate of cultural Zionism in the United States. Borrowing from his spiritual mentor, Ahad Ha'Am, he perceived that modern Jewish identity was a potent bulwark against communal disintegration. Over the course of the succeeding decades, the messages that he articulated have become basic building blocks of a mixed civic-religious Jewish identity professed or experienced by the majority of acculturated American Jews. Many of the prime expositors of this identity were Friedlaender's colleagues and students. But, in the lists of the most important contributors to the development of this point of view, the name of Friedlaender is all too often omitted.

Baila Round Shargel is to be commended for recognizing Friedlaender's historical importance. Her well-researched biography details the personalities with whom he was embroiled, the issues that energized him, and the ideological roots of the positions he took during his tragically shortened American career. Future researchers of American Jewish religious and intellectual history will be sure to include Friedlaender in their considerations.

The only serious drawback of Shargel's work is that it is seemingly directed at a limited group of experts. Shargel assumes too much from the reader. Moreover, issues of limited interest to any but specialists are given extensive coverage at the expense of problems that would appeal to a wider audience. One example should suffice: more than five pages are devoted to the withdrawal of Friedlaender's invitation to a Red Cross expedition to Palestine in 1918. The answer to that question, which Shargel herself ultimately admits remains a mystery, would be of concern to Friedlaender scholars, but probably to no one else.

JEFFREY S. GUROCK
Yeshiva University

DAVID E. NYE. *Image Worlds: Corporate Identities at General Electric, 1890–1930.* Cambridge, Mass.: MIT Press. 1985. Pp. xiv, 188. $20.00.

Formed in a merger of Edison General Electric and the equally successful Thomson-Houston Electric Company in 1892, General Electric (GE) dominated the American electrical industry and was largely responsible for the electrification of the United States during the early decades of the twentieth century. GE was certainly one of the most innovative, powerful, and influential of the new technology-based coprorations that came to dominate the American economy between 1880 and 1930, yet its impact on the nation's social and economic structures has scarcely been recognized. As David E. Nye points out, historians too often assume that the form and functions of large corporations derive from something inherent in their technology and in the development of modern capitalism, so that they are seen more as a result than a cause of historical forces. Basing his study on photographs selected from GE's archives and on a careful reading of the company's advertisements and in-house magazines, Nye set out to show that, for one corporation at least, many of those forces came from within. In this highly innovative study he achieves a powerful synthesis of the "myth and symbol" approach of American Studies with that of social history. The result is a complex picture of the corporation as shaper of values, molder of roles, and effective promoter of the type of national culture that best met its needs.

Nye's primary concern is with the subtle and not-so-subtle messages of the photographs used in company publications intended for different types of employees and for the public. Analyzing the form and content of these photos, Nye is able to demonstrate convincingly that the company presented entirely different images to its workers, managers, engineers, and customers. This finding would not be particularly surprising except that the images sometimes contradicted one another. To its workers GE showed a vision of harmonious, nonunionized relations, with possibilities for advancement limited to standings in the company bowling league. Little was said about the job itself; satisfaction came from social relations with coworkers. For its managers GE

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was a caring family whose members were expected to cooperate for the good of the company while clawing their way to the top. By emphasizing the scientific component of engineering work, company publications made its engineers feel apart from the hands-on development that actually characterized much of their work. This alienated them from the workers and brought stronger identification with the needs of management. And, finally, GE presented itself to the consuming public as the provider of a technological social progress measured in innovative consumer goods.

Through his insightful analysis of both form and content of numerous photographs, Nye reveals how these and other messages were transmitted to the appropriate groups. Whether describing the pictured relationship of workers to their machines—presented differently in the photographs intended for engineers and for managers—or the Americanization classes and intramural sports teams shown to workers, Nye demonstrates that photographic techniques transmitted certain vital "truths" to selected audiences. "The individual photographs each seemed to be objective statements of facts such as the audience knew them. They concretized otherwise unspoken assumptions about the social order and established work hierarchies, social roles, and group attitudes" (p. 155). Nye also examines GE's advertising during the early twentieth century and demonstrates that pictorial materials were used to influence taste and allegiance to brands and to promote acceptance of a fully electric society dominated by private power companies. This historical approach, combined with appropriate social and economic analysis, makes clear the ability of the progressive corporation to influence the values of its employees, customers, and regulators—in short, the values of the society in which it operated.

This well-conceived and carefully researched book covers an important topic and deserves a wide audience among historians. My only quibble is that pictorial materials are gathered at the ends of the chapters without reference numbers, which makes it awkward for the reader to relate the photographs to the text. A more imaginative design would have done the book justice.

LEONARD S. REICH
Colby College

JOSEPH R. FRESE and JACOB JUDD, editors. *Business and Government: Essays in Twentieth-Century Cooperation and Confrontation*. Tarrytown, N.Y.: Sleepy Hollow Press, for Rockefeller Archives Center, Pocantico Hills, New York. 1985. Pp. xii, 233. $25.00.

A product of the Fourth Conference on American Economic Enterprise at the Rockefeller Archives Center in 1984, this book, edited by Joseph R. Frese and Jacob Judd, is primarily concerned with the first half of this century, more particularly the 1920s and 1930s. As the series subtitle implies, the authors address both government regulation of and cooperation with business to meet the demands of the modern American economy.

In a challenging chapter, "Foundation Stones for a New History of Business-Government Relations in Modern America," Louis Galamos suggests that historians give up the old "progressive" and "conservative" explanations of those relations in favor of studying the institutions—some government, some private, some a mixture of the two—that in this century have enhanced economic security while encouraging economic growth and efficiency. In presenting his case, Galambos surveys agriculture and the professions as well as business.

Robert F. Himmelberg, writing on government and business from 1917 to 1932, examines the concept of "corporate liberalism" (the idea that business might regulate itself through trade associations and that government interference be held to a minimum) as it has evolved from the work of recent assessors of the 1920s, including biographers of Herbert Hoover. Himmelberg concludes that in that decade "a corporate vision [was developing] . . . but the vision looked less like a concert of interests [among business groups] than a stand-off between powerful functional or interest blocs" (pp. 19–20). Ellis Hawley, moving on to the administrations of Franklin D. Roosevelt, discerns a period of cooperation between business and government in the early New Deal, followed by a period of conflict (which reached its peak in the period 1935–38) and then by a second period of cooperation. Hawley places the Roosevelt years within a context of seesawing adversarial and cooperationist periods of government and business relations in the twentieth century and avers that many of the institutional innovations in modern America came out of the cooperationist impulses.

The remaining four chapters in the book, by Albro Martin, Mira Wilkins, David R. B. Ross, and Arthur M. Johnson, treat more narrowly defined topics. In "The Self-Inflicted Wound: Government Regulation and the Destruction of the American Railroad System" Martin repeats the indictment of government railroad policy that he presented in his *Enterprise Denied: Origins of the Decline of American Railroads, 1897–1917* (1971). Wilkins, in a chapter on the role of the federal government in foreign direct investment in the United States from 1918 to 1941, concludes that such investments in this country were sizable in those years but that it "grew in spite rather than because of U.S. government policy" (pp. 104–05). Ross examines the historical controversy concerning the development of synthetic

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rubber in the United States before Pearl Harbor. Did industry, especially the Standard Oil Company (New Jersey), retard that development, or was the government responsible for the delays that caused "a rubber panic" in 1942? Ross points out that, although the low price of natural crude rubber in the 1930s made the necessary technological innovation for development of the synthetic product unattractive to business, the government must be blamed for not actively encouraging, even demanding, such development in the national interest. Finally, Johnson's chapter, "The Federal Trade Commission: The Early Years, 1915–1935", examines one of the institutions that Galambos urges should be studied. Johnson finds that, although uncertainty as to its jurisdiction, hostility of the business community, and limitations imposed by the courts all hampered the activities of the Federal Trade Commission, the agency nonetheless "pioneered pragmatically in showing both the strengths and shortcomings of business regulation by commission" (pp. 182–83).

Taken as a whole, the essays in *Business and Government* open a number of windows on an understanding of that important subject up to World War II. What is needed now is a conference that will supply us with a book on the role of the federal government in the economy since what Herbert Stein has called "the fiscal revolution" of the 1940s.

IRENE D. NEU
Indiana University

EUGENE M. TOBIN. *Organize or Perish: America's Independent Progressives, 1913–1933*. (Contributions in American History, number 114.) New York: Greenwood. 1986. Pp. xiv, 279. $37.50.

American reform movements are both fascinating and frustrating. They are fascinating because reformers are driven to consider the fundamental questions confronting human beings in their relationships with one another. They are frustrating because reformers have seldom achieved the success through their organizations that they have dreamed of as individuals. In this careful and insightful study Eugene M. Tobin examines the ideas and activities of progressive thinkers in American politics during the years between Theodore Roosevelt's Bull Moose campaign and Franklin D. Roosevelt's New Deal. In reading the book, one shares the author's fascination with the activist liberal reformers as well as the reformers' frustration with repeated failure. Through the style and sensitivity of the narrative, however, the fascination prevails.

Beginning with independent progressives such as Amos R. E. Pinchot and George L. Record, whose hopes for fundamental change in 1912 predominated over their commitment either to Roosevelt or to the Progressive party, Tobin proceeds to clarify the differences that emerged within the liberal community prior to American entry into World War I. His skillful probing of relationships between the reformers and organized labor represents an important contribution to labor history as well as to the history of reform. Most independent progressives, he points out, were "patronizing, arrogant, and condescending in their attitudes toward organized labor" (p. 61). Although they battled for workers' rights, they had difficulty in providing support for a strong union movement.

The problem of developing an effective relationship with labor continued to plague the liberals throughout this period, but after 1914 their attention focused on efforts to prevent American involvement in war. Always opposed to privilege, they opposed the gains of special interests from preparedness and from the war itself. After the war the independent progressives attempted to organize the Committee of Forty-eight to prevent privilege from dominating American politics. They avoided associating themselves with the National Labor party, however, because they believed that labor leaders did not understand their motives in seeking the abolition of privilege.

Most of the independent progressives backed Robert M. La Follette when he ran for president in 1924, but, in hewing to principle and to reasoned attacks on privilege, they again demonstrated their ineptitude as practical politicians. During the "wilderness years" between 1924 and the New Deal, the independent liberals carried on their efforts despite defeats and discouragement. New leaders emerged, along with new organizations such as the National Citizens' Committee on Relations with Latin America and the League for Independent Political Action. Yet the efforts of the liberal reformers remained a constant, and Tobin sees their persistence as the key to understanding their importance.

What drove them on? Pinchot answered the question when he wrote a friend that "the main point which I make my apology for my existence is not that I have been right about things, but that I have tried hard to make people take an interest in public problems" (p. 218). That objective of concerned progressives, like their commitment, commands the author's respect and ours.

Tobin has performed an important service in explaining both the steady course of the independent progressives and their often convoluted political activities. His study is intelligent and valuable.

PAUL W. GLAD
University of Oklahoma

MICHAEL PEARLMAN. *To Make Democracy Safe for America: Patricians and Preparedness in the Progressive Era.*

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Urbana: University of Illinois Press. 1984. Pp. 297. $19.95.

In this ambitious intellectual and social history Michael Pearlman scrutinizes the flip side of the preparedness movement of World War I. Examining some forty-five key leaders of such groups as the National Security League, the American Defense Society, and the Plattsburg Training Camps Association, Pearlman contends that these people agitated less to achieve needed military reforms against international dangers than to secure an older vision of America against the alleged disintegrative effects of industrialism and a marketplace economy. By linking the movement for universal military training to contemporary developments in public health, scouting, higher education, and even immigration, the author shows that the elites who urged preparedness sought no less than a renewal of national cohesion through the spartan regimen of military service. For Henry L. Stimson, Theodore Roosevelt, Jr., and a dozen other officers of New York City's Seventy-Seventh Division in World War I, service in the so-called Melting Pot Division constituted "the national idea carried into practice," wherein Americans of diverse backgrounds acquired a common military bond: "The Jews and the Wops, and the Dutch and the Irish cops; they're in the Army now" (p. 150). Using interdisciplinary insights, Pearlman even connects the ethos of preparedness to such eccentric reforms as George Wharton Pepper's philanthropy in behalf of the Washington Cathedral and William Temple Hornaday's transformation of the Bronx Zoo.

Although critical of repressive tendencies in the ideology of preparedness, Pearlman nonetheless empathizes with patricians who tried to practice what they preached. Probably his most poignant portrait is of Roosevelt, whose struggle to live up to his father's strenuous ideals led to heroic service in both world wars (and earned him the Congressional Medal of Honor) but not to the expected warrior's death in battle; Roosevelt died of a heart attack shortly after D-Day. Pearlman makes the excellent point that Roosevelt, exemplifying the citizen-soldier spirit of San Juan Hill and Plattsburg, found himself out of step with the rigid professionalism of the mass army of World War II. Indeed, as this fine book reminds us, the most important changes in American military policy have come at the initiative of civilians, and these changes usually reflect larger sociopolitical changes in American society and in the international system.

J. GARRY CLIFFORD
University of Connecticut, Storrs

MIROSŁAW FRANČIĆ. *Komitet Obrony Narodowej w Ameryce, 1912–1918* [The Committee of National Defense in America, 1912–18]. (Biblioteka Polonijna.) Wrocław: Ossolineum. 1983. Pp. 228. 170 Zł.

Before 1918 the Poles of America regarded themselves as members of a political community centered in partitioned Poland, particularly Austria, where Polish politics were relatively untrammeled. Developments there were reflected—after a delay and with increasing distortion—in the United States. In 1912 the threat of a European war resulted in temporary unity among Austrian Poles. In the United States a derivative all-Polish coalition appeared: the National Defense Committee (or KON, its Polish acronym).

The KON (1912–21), despite its claim to represent the entire spectrum of Polish politics, was in reality the work of a minority: the portion of the Polish political Left that was dedicated to the defeat of Russia and was led by the charismatic Józef Piłsudski. Within months after its founding, the KON began to disintegrate as political opponents defected to the Right. After 1914 it probably represented only 10 percent of organized Polonia. Because it sought the defeat of Russia, the KON was sympathetic to the Central Powers. Hence, after America's entry into the war in 1917, the position of the KON was untenable, and its activities virtually ceased.

On the basis of Polish, Austrian, American, and Polonia archives, Mirosław Frančić has reconstructed the history of the KON. Frančić addresses a topic largely ignored by historians who have tended to concentrate on the more successful activities of Ignacy Paderewski and his supporters on the Polish Right. Usually the KON is dismissed as a noisy minority of radicals, agents of the Kaiser, or, *mirabile dictu*, both. Instead, the KON was a complex combination of several elements within Polonia: eastern seaboard Poles from Russian or Austrian Poland as distinct from the dominant midwestern Polonia establishment controlled by Poles from Germany; the political Left; and religious dissenters. Most important, the KON represented the least assimilated element of the Poles in America, those most attuned to European rather than parochial Polonia developments.

In general, Frančić is reasonable in his judgments and displays admirable knowledge of both Polish and Polonia politics. In recounting the history of the KON, however, he occasionally loses sight of the larger political and social context. A number of issues are sketched very broadly or ignored. The fundamental questions about the KON are who supported it, why, and what the answers divulge about the structure of the Polish community in

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America. Although Frančić's adumbrations in this regard are plausible, they are too superficial.

In sum, this work is of signal value to specialists in Polish history and helps fill a lacuna in the historiography of the international efforts to reestablish an independent Poland. More importantly, it raises basic questions concerning the Poles in America that historians and sociologists have hitherto ignored.

M. B. BISKUPSKI
St. John Fisher College

KRISTIN THOMPSON. *Exporting Entertainment: America in the World Film Market, 1907–34*. London: British Film Institute; distributed by University of Illinois Press, Champaign. 1985. Pp. xiii, 238. $24.95.

In the early twentieth century American motion pictures became prime avenues of cultural imperialism. If Henry Luce's much-publicized "American century" had any validity, the hugely profitable Hollywood cinema certainly formed a part of this phenomenon. Concentrating on the distribution end of the film business, Kristin Thompson has produced a densely packed statistical analysis of American film hegemony throughout the world.

The author maintains that aggressive American marketing procedures set the pattern for this dominance even before the usually accepted time of World War I. Prior to 1914 U.S. filmmakers faced two major challenges: to reduce the foreign share of domestic markets and to establish agents and subsidiary offices abroad. Between 1908 and 1915 the notorious Motion Picture Patents Company (MPPC) went far toward achieving the first goal. Because films were inexpensive to make and the market was expanding rapidly, however, its attempt at monopoly never completely shut out either foreign or domestic competition. Still, the MPPC succeeded in excluding all but a few foreign firms, and the others had great difficulty regaining their American distribution. Government tariff policy played almost no role in these freewheeling international markets.

Also before the war American producers began a systematic development of foreign markets, which was checked only slightly by the war itself. By the mid-1920s most firms had representation in virtually all viable overseas areas. Britain was the best market, Germany second. Producers initially were less successful closer to home, in Central and South America, although after the war cinematic cultural domination south of the border made great strides. Australasia succumbed readily to American films, but Africa and the Orient remained backwaters.

The war cut deeply into the product of the leading European producers, France and Italy, and aggressive American agents moved quickly to fill the gap. Even Germany continued to import American film (through Scandinavia) as late as 1916. Conversely, the war caused a gradual reduction in the significance of foreign-produced films in the American market. The central role of London in the overseas distribution of American movies similarly was eroded.

After 1918 Germany, Britain, and France tried to fight American dominance with quota systems, with but slight success. These economic grounds for opposition were shared by almost every other country in the world, with the exception of Mexico, which, angry over the stereotypical treatment of its nationals in *yanqui* films, fought the celluloid invasion from the north on ideological grounds. By the end of the 1920s America was firmly established in nearly all world markets, the major exceptions being the Soviet Union, Germany, and Japan. The sound revolution produced little change in this situation.

The strengths of Thompson's arguments are also drawbacks. Her narrow, in-depth focus and her useful, thorough statistical support, drawn mostly from trade periodicals, well illuminate this most neglected facet of the film industry. The analysis is presented in straightforward, if dogged, style and sometimes verges on a catalogue of statistics. Because of her concentration on distribution, the author gives few clues as to why the American product proved so popular so quickly in so many places. Above all, was the product itself or its marketing more important?

Nevertheless, as a compact survey of a crucial and not widely studied branch of the film industry, Thompson's work makes a strong argument against relying solely on historical analyses of "national" cinemas. Thus, this book is a useful addition to the shelf of the film scholar and the cultural historian.

MICHAEL T. ISENBERG
United States Naval Academy

WILLIAM M. LEARY. *Aerial Pioneers: The U.S. Air Mail Service, 1918–1927*. Washington, D.C.: Smithsonian Institution Press. 1985. Pp. 309.

Long before the government assumed regulation of civil aviation in the United States (1926), a number of brave pioneers in the military, the post office, and civilian aviation experimented with (1911) and finally established (1918) the first scheduled air mail service in the world. The struggle was not easy, and veteran aviation historian William M. Leary skillfully relates the battle involving pilots, bureaucrats, and Congressional whims over appropriations, all within the context of a most primitive status of aviation technology.

Leary uses previously untapped government and private correspondence. Personalities served often to augment or hamper the development of aviation.

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Otto Praeger, the relentless organizer, demanded that pilots fly at all times and relegated safety to a secondary consideration; Paul Henderson insisted on safety first; and Stephen A. Cisler emphasized both safety and organizational efficiency. Daredevil and careful pilots take center stage, and rightly so, while the author gives vivid descriptions of the services: New York–Washington, D.C.; overnight flights between New York and Chicago; and the more celebrated transcontinental route. Thirty-four pilots died, some through human error, several because of adverse weather or inadequate supporting aids, more because of technical flaws in such airplanes as the DH-4 (deHavilland) and the Douglas M-4. Certain politicians advocated the use of only veteran military pilots, whereas the more-often prevailing majority favored civilians hired by the post office.

Well-written and attractively designed, this book contains few and only minor shortcomings. Chapter 10 on Aeromarine Airways, an excellent previously published article, presents a tacked-on appearance, which mars the easy flow of the main topic for the reader. Leary is correct in his favorable assessment of the contributions of the Post Office Air Mail Service to the later federal control under the Aeronautics Branch and its successors, but he exaggerates this role, for several other private and military developments in aviation were probably just as important. He might have better documented the assistance of the Weather Bureau to the Air Mail Service. The author could have cited parallels of this study to later periods of aviation. For example, Praeger's insistence that pilots fly regardless of weather compares with the similarly poor safety record induced by zealous commuter airlines, especially since 1975. The excellent delivery speed of air mail in these early years could be contrasted to our slower air mail service of the most recent years. More clarity might have been sought over the status of American versus European aviation, a perennially difficult research question.

Nevertheless, Leary's book is a model study, well augmenting existing works on other aspects of aviation since 1918. It will prove useful to general historians, aviation enthusiasts, and students of administrative history.

DONALD R. WHITNAH
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The Women of Summer: An Unknown Chapter of American Social History. Produced by SUZANNE BAUMAN and RITA HELLER. Washington, D.C.: National Endowment for the Humanities, distributed by Filmakers Library, New York. 1985. 55 minutes. Film $850.00, video $700.00, rental $85.00.

From the opening scene of whirling factory gears to the closing shot of former students and faculty of the Bryn Mawr Summer School gathered at Bryn Mawr College for a reunion in the summer of 1984, this film commands attention. The Bryn Mawr Summer School, an experiment in workers education, was the idea of M. Carey Thomas, president of Bryn Mawr College, and the project of Dean Hilda Worthington Smith, whose dedication and patience made the school a reality. Each summer between 1921 and 1938 Bryn Mawr brought together one hundred female industrial workers from across the United States for a two-month term. Recruited by local committees of Bryn Mawr alumnae, the students received scholarships to study at the school, where a predominantly female faculty of leading labor economists, historians, and English professors taught interdisciplinary courses designed for the worker-students.

Students came from a variety of backgrounds, but all shared the effects of having entered factories between the ages of ten and fourteen, working twelve-hour shifts, and taking home meager wages to help support their families. Students arrived on the campus suspicious of each other and of Bryn Mawr itself. Passing through Rockefeller Arch the women questioned whether the Rockefellers owned the school, and streetwise students warned themselves not to "get to like this too much." One student recalled that she came with "plenty of chips on my shoulder." But, in the open and supportive atmosphere of the school, students agreed that the chips "began to fall off."

The film is visually appealing and historically accurate. A documentary true to its subject and at the same time lively, fast-paced, and emotionally uplifting is a rarity, and this one does it all. The juxtaposition of archival footage of students at the school in the 1920s and 1930s, still photographs, and newsreels of the Bonus Army and strikes of the 1930s is skillfully accomplished. The reunion of summer school students and faculty in 1984 provides the perfect focus for the film and a unique and powerful fifty-year retrospective that confirms the profound impact of the school on individual lives.

The documentary draws heavily on oral testimony by alumnae to present the history of the school from their perspective, but the film makers also have woven personal reminiscences, letters, school documents, speeches, diaries, government studies, and newspaper coverage into a rich tapestry that places the school in the larger context of American history between the two world wars. Esther Peterson, a vibrant former faculty member, narrates much of the film, which skillfully integrates historical footage and voiceovers with discussions between former students and faculty.

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The emotional impact of this narrative film is tremendous. The producers, award-winning film maker Suzanne Bauman and historian and Bryn Mawr alumna Rita Heller, are understandably sympathetic to the women whose story they tell so well. Appropriately, Bauman and Heller emphasize positive aspects of the school's history and relate the biographies of successful former summer school students. The bias in selection that is built into any long-term follow-up is always a problem and in this case tends to glorify the summer school experience. But the women who did not become activists or remain part of the Bryn Mawr network are inaccessible, and we can only wonder about the impact of the school on the majority of the seventeen hundred women who attended.

Because the producers allow former students to speak for themselves, the film deals with the experience of working women in the 1920s and 1930s in a realistic, nonstereotypical, nonromantic manner. The complexity of these women's lives, the richness of their humanity, and their strength come across clearly. To the credit of the film makers, the former students dominate the film and set the tone, as one can easily imagine their having done at the school when they demanded better quarters for the household staff and 50 percent representation on the governing board of the summer school.

The film portrays the school as above all a women's community in which immigrant women, black women from the rural South, trade union activists, and workers from nonunion shops lived apart from the dominant patriarchal society and thrived in a nonhierarchical atmosphere. As the film proceeds, former faculty members testify regarding their respect for the summer school students and the stimulation of teaching women whose life stories "came pouring out" and became the basis for classes in economics and labor history. Students fondly remember how a nonroutinized curriculum with open discussions enhanced a vital women's culture that flourished as women wrote plays and poetry and devised their own schema for economic recovery during the depression. Students recall sharing traditional music and dances in celebration of their ethnic diversity. Peterson, other faculty, and students describe the school as a "salon" for the labor movement and the Left that brought together avant-garde women and men, including W. E. B. Du Bois, Norman Thomas, Walter Reuther, Margaret Sanger, Frances Perkins, and Eleanor Roosevelt, who spoke to the students and joined in informal discussions with groups of faculty.

Although the film emphasizes the solidarity shared at the school, the producers also address the conflicts between Bryn Mawr College and the summer school, which flared almost annually. Bryn Mawr trustees and wealthy supporters did not mingle with industrial workers who were invited for the summer. On the contrary, they complained when the unemployed relatives of some of the students slept on the floor of the Bryn Mawr railroad station; they objected strenuously when five black women were accepted by the school in 1926; and they demanded that students and faculty who attended strikes and protests participate as individuals only, not as representatives of the summer school. When the concept of gradual change endorsed by female reformers in the 1920s gave way to the heightened political activism of the 1930s, one trustee revealingly asked "Why should we support your organizing workers to strike our husband's plants?" Thomas's and Smith's cherished vision of feminism transcending class differences shattered completely when the trustees disapproved of the involvement of members of the summer school in a strike at nearby Seabrook Farms and forced the school to leave Bryn Mawr in 1938.

If anything, however, the producers probably underemphasize the tension inherent in such an enterprise. Smith clearly had a tiger by the tail, and the toll on her was significant. She wrote in her autobiography that directing the summer school often left her "confused, fatigued, almost in despair." As the film makes clear, the faculty members, who came from middle-class backgrounds, acted as intermediaries, recruiting workers and raising funds from the wealthy. Actually, the Bryn Mawr Summer School's history is remarkable not in that the board of trustees forced the summer school to leave Bryn Mawr in 1938 but in that such an institutional alliance worked at all.

Unlike other films of this genre that self-consciously downplay the influence of the Left from a post-McCarthy perspective, "Women of Summer" openly acknowledges both the political energy and intellectual rigor that socialists and communists brought to the school. Political ideology was openly discussed, courses were taught on Marxism, and an individual's choices for ideological alignment included a variety of factions on the socialist Left. The film also emphasizes the urgent necessity for action engendered in the worker-students by the desperate economic conditions of the Great Depression, a period when, as one student recounts, "it was easy to be very extreme." But eventually, as Peterson calmly explains, "There was a red scare, there was no doubt about it," and in the view of those who funded the summer school and ran the college, "we weren't just nice girls anymore, we were vigorous people who wanted to change society."

The film proudly recounts the accomplishments of former summer school students in the labor movement, and, although many workers who came to the school were already grass-roots leaders in their unions or factories, the skills and experiences

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gained at Bryn Mawr often enhanced their careers. The film does not explore the often-tortured relationship between the labor movement and the Bryn Mawr Summer School. Some unions viewed the school as an institution that bought off workers with a temporary taste of the good life, others grew suspicious of the influence of leftists at the school, and all resented the school's emphasis on women. The difficulties of reconciling class and feminist interests troubled many female unionists who sought to strengthen the role of women in male-dominated unions by taking advantage of the opportunities provided by the middle- and upper-class women who ran the Bryn Mawr Summer School.

This carefully researched film does not view the feminism of the 1920s through a contemporary feminist lens. The culture of this women's community is carefully reconstructed on its own terms. Consequently, the past is not distorted to confirm a particular present. Rather, the links between the 1920s and 1930s and the 1960s and 1970s are dealt with implicitly, almost casually, conveying an important and natural sense of continuity. Many of the women interviewed in the film, Peterson, Alice Cook, and Caroline Ware, for example, provided crucial support to the contemporary women's movement, but their activities, and those of former students in recent decades, are scarcely mentioned. This is understandable, although a connection could have been drawn to the current summer schools for union women sponsored by the University and College Labor Education Association, the Coalition of Labor Union Women, and the AFL-CIO Department of Education. The organizers of these contemporary schools have consciously modeled their programs after the Bryn Mawr Summer School.

The film will make a valuable contribution to courses in women's and oral history, and to general survey and upper-division courses on the 1920s and the New Deal. For both traditional and adult students, the film raises important questions about the effects of industrialization, class divisions, ideological commitment, and the role of women in American society. The Bryn Mawr Summer School gave many women the strength to become historical actors. In this educational experiment women taught each other that they had the power to shape events and, most importantly, their own lives. Heller and Bauman have ensured that the story of the Bryn Mawr Summer School will be preserved, and in the process they have crafted a beautiful film. Historians and students of the American past are significantly richer for their efforts.

MARY FREDERICKSON
University of Alabama, Birmingham

BARRIE A. WIGMORE. *The Crash and Its Aftermath: A History of Securities Markets in the United States, 1929–1933*. (Contributions in Economics and Economic History, number 58.) Westport, Conn.: Greenwood. 1985. Pp. xx, 731. $49.95.

In this hefty volume Barrie A. Wigmore has written an exacting and detailed history of American securities markets during the Great Depression. The centerpiece of this study is an examination of the performance of leading stocks and bonds. To carry out his analysis, Wigmore has assembled an impressive data base of annual information on 142 companies. This includes data on earnings, prices of securities, book values, leverage, cash holdings, dividends, yields, and Moody's bond ratings. Chosen on the basis of their shares of trading volume and market value, the stocks represent about 77 percent of the market value of all New York Stock Exchange stocks. These valuable cross sections for each year of the depression are published in a statistical appendix along with other financial and macroeconomic data. By combining his analysis of these statistics with a history of the industries and firms, Wigmore has produced what will prove to be a very useful handbook. Any scholar interested in the fate of a particular industry or a major firm during the depression will find it most helpful.

Although the features of the general decline in stock prices from 1929 to 1933 are well known, Wigmore's examination of individual stocks is revealing. Some companies continued to perform well during the depression, and twenty-three of his sample firms maintained their dividend rates. Within particular industries good management in the face of adversity was important. As one would expect, firms that were able pay their stockholders reasonable dividends maintained the prices of their stock. The best stocks were in gold and tobacco. Deflation benefited firms that handled these commodities by reducing their costs; furthermore, the real price of gold rose and tobacco consumption remained stable. The severe decline in the value of the stock of financial companies (holding companies, investment companies, and banks) emphasizes the financial nature of the crash and the depression.

In general, the bond markets fared better. Most corporate and foreign bonds rated "A" or better recovered most of their value by 1933. With the particular exception of South American and Eastern European bonds, which suffered numerous defaults, the issues rated "Baa" regained much of their value after the initial crash. The huge decline in prices simply made attractive the fixed nominal payments on bonds with low risk of default. The safety of U.S. Treasury securities caused their prices to rise during the depression. Wigmore calculates

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the real rate of return on holding these bonds to have been over 40 percent.

The analysis of the stocks and bonds is accompanied by a straightforward chronicle of the national and international economic events affecting the crash. Wigmore draws on the standard sources for this background material but offers his own assessment of the causes of the depression and the role of monetary policy. He believes that the depression was brought about by a succession of basically unrelated shocks to the economy, including the crash of the stock market, the banking panics, and international currency crises. Most economists believe that the Federal Reserve System's failure to stem the decline in the stock of money was an important factor contributing to the collapse of the economy. Wigmore, however, does not blame the Federal Reserve because he doubts the ability of an expansionary monetary policy to stimulate the economy. His objection is that additional bank reserves would not have induced banks to increase their lending and purchases of securities. Although this may have been true in early 1933, when confidence in the economy had virtually evaporated, earlier aggressive action by the Federal Reserve would certainly have had a strong positive effect. For bringing the economy out of depression, Wigmore gives credit to the National Industrial Recovery Act, the Agricultural Adjustment Act, and other measures. Yet, most economic historians would argue that these actions had little effect because they only served to change relative prices and failed to increase aggregate demand.

Although the analyses of policy and the causes of the depression are somewhat weak, this should not detract from Wigmore's achievement. This book is a welcome guide for those interested in the fate of American securities markets during the Great Depression.

EUGENE N. WHITE
Rutgers University

JANET POPPENDIECK. *Breadlines Knee-Deep in Wheat: Food Assistance in the Great Depression.* New Brunswick, N.J.: Rutgers University Press. 1986. Pp. xvii, 306. $30.00.

This is a case study of the New Deal's "failure" to revolutionize the American economic system. The point, of course, is not a new one. It was made in the 1930s and again in the 1960s. Janet Poppendieck identifies her interpretation with two major proponents of this view in the 1960s, Howard Zinn and Barton Bernstein. Furthermore, agricultural policy is not a new source of evidence of the nonrevolutionary character of the New Deal. Yet, proponents of such an interpretation of the New Deal for agriculture have most often relied on treatment of the rural poor, especially the southern sharecroppers, to support their case. Poppendieck relies on the farm program's treatment of impoverished consumers, most of whom lived in cities, to give the largest, most thoroughly researched examination that has been made of food assistance in the Great Depression.

Poppendieck deals with the efforts in the 1930s to come to grips with what was regarded at the time as a paradox—and something new in history: the coexistence of a highly productive agriculture and hungry people. ("Breadlines knee-deep in wheat," was, she indicates, the way this was often expressed.) She begins with agriculture before the Great Depression, chiefly to show how an attitude that dominated farm policy in the 1930s took shape. Then, after examining the impact of the depression on farmers and workers, she looks at the pressure for action on behalf of the hungry during the Hoover presidency. She calls attention to the great resistance to food distribution, especially out of fear of its impact on commercial farmers, but shows that distribution of Farm Board wheat became the first federal relief program for the unemployed. Next, she describes the situation when Roosevelt came to power and emphasizes the potential for large-scale change. Moving on to a substantial discussion of New Deal food assistance programs, she argues that they originated in the pig-killing activities and were transformed in aim from aiding the poor to raising farm income after their transfer to the Department of Agriculture in 1935. Finally, surveying the New Deal's legacy for the future, she stresses its negative features but reveals that it had a positive side as well.

Could more have been accomplished? Poppendieck appears to think so, although she also shows that the distribution of power placed limits on the New Deal, and she seems ambivalent about the need of the commercial farmers for government help. Furthermore, she demonstrates that change did take place. For the first time, the federal government did distribute food to needy people, and the experience provided background for later and better efforts. A scholar with a different philosophy would make more out of these facts. The author is surely correct in arguing that the New Deal did not revolutionize the system of food distribution. That it modified the system is also clear. That the opportunities for change were substantially larger then the change the New Deal promoted is not. The farm program could have been given a different shape if, as she suggests, the Roosevelt administration had worked more closely with representatives of consumers and the rural poor and less closely with agents of the commercial farmers; yet the possibility

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of doing so was not large, however great the benefits, then and later, might have been.

RICHARD S. KIRKENDALL
Iowa State University

MARLENE PARK and GERALD E. MARKOWITZ. *Democratic Vistas: Post Offices and Public Art in the New Deal.* Philadelphia: Temple University Press. 1984. Pp. xx, 247. $37.95.

Among the emergency programs and experiments inspired by President Franklin D. Roosevelt, social historians generally give high marks to the projects that employed artists, actors, musicians, and writers. At the very least it is agreed that, by diverting a small percentage of the government's welfare and construction dollars into work projects for practitioners of the arts, Roosevelt saved their skills from atrophy. Marlene Park and Gerald E. Markowitz are among a growing number of scholars who see in the various New Deal arts projects an effort to create a distinctly American culture—one that was democratic, spiritually uplifting, and characterized by communal symbols that might contribute to unity and help people make it through the Great Depression. In this book the authors assess the contributions of the Treasury Department's Section of Painting and Sculpture, later renamed the Section of Fine Arts. This subunit of only nineteen federal employees awarded almost fourteen hundred commissions to about eight hundred fifty mural artists and sculptors.

Established by Secretary of the Treasury Henry Morgenthau, Jr., in October 1934, "the section," as insiders and artists called it, differed significantly from the much larger and better-known welfare arts projects (Works Progress Administration) in that it existed to decorate new federal buildings with the best murals and sculpture America's artists could create. Led by businessman-turned-artist Edward Bruce, the section was free to engage artists of its own choosing, not merely those who were destitute.

With a stable budget, artists hired strictly on merit, and the interior walls of practically every new post office and federal building on which to display their art, administrators of the section turned to resolving what Park and Markowitz call "tensions between three polarities" (p. 9). How could the section at once achieve quality in art and make art democratic? How could its commissioned works project national ideals and still reflect the interests of people in each locale? How could the section assure adherence to traditional artistic values and foster the styles that were popular in the 1930s? No clear answers emerged, but the murals in federal buildings testify to the fact that Edward Bruce tried. By intervening while the artists executed their work and by withholding a substantial portion of the payment until each mural was satisfactorily completed, Bruce imposed the values he perceived most important to New Deal America.

Park and Markowitz offer the most perceptive exposition of the message and style of section murals in print. They see in the vast number of murals on historical topics a past characterized by peace, production, and progress. They see an emphasis on "the people" and note the recurrence of happy families, heroic workers, and reassuring scenes of abundance. They identify regional themes and argue that the section brought artists of disparate aesthetic and political views into a typical New Deal coalition. In this impressively researched and illustrated volume Park and Markowitz have bridged the sometimes-awkward gap between social history and art history.

RICHARD MCKINZIE
University of Missouri, Kansas City

WILLIAM BRUCE WHEELER and MICHAEL J. MCDONALD. *TVA and the Tellico Dam, 1936–1979: A Bureaucratic Crisis in Postindustrial America.* Knoxville: University of Tennessee Press. 1986. Pp. xii, 290. $34.50.

The Tennessee Valley Authority (TVA), once a proud example of New Deal government enterprise, has fallen on disastrous days. Its nuclear power plants have been idled by safety tests, its water supply lowered by drought, and even its environmental activities have been questioned. William Bruce Wheeler and Michael J. McDonald have written this new book on a controversial episode that pinpoints the organization's contentious nature.

I recall as a boy fishing on the tranquil banks of the Little Tennessee River, which feeds the Tellico Dam. From then until I wrote a history of the TVA, the valley changed from a primitive, but wholesome, land of farms and woods to a thriving, semi-industrial country, transformed mainly by the TVA. Although much of the present volume is mainly of local and regional interest, the book portrays some infighting of the organization that gained national attention with its furor over a small, native fish.

In November 1979 the TVA closed the gates on the 136-million dollar dam and thus impounded the last free-flowing stretch of the river. For the forty-year-old project, thirty-eight thousand acres of land had been purchased, sixteen thousand were soon flooded, and four thousand were set aside for a lakeside resort and for industry. The result, according to the authors, was "a Veblenian dream turned nightmare in the hands of politicians" (p. x). On the other hand, it was an ideal dream of longtime TVA Chairman Aubrey Wagner who represented the

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best traditions of the organization. His reaction to this book, expressed in a letter to me, was: "The authors are part of the UT crowd who tried making their reputations by opposing TVA and Tellico, and all they accomplished was to add 70–80 million dollars and delay the project."

Local opposition had reached an uproar. Here the authors take a rather condescending attitude toward people of eastern Tennessee. Most residents, however, envisioned new jobs and prosperity and favored the project. But leading much opposition was diminutive but fiery Alice Milton of Chattanooga, who was director of nearby historic Fort Loudoun. Graves of Cherokee ancestors were inundated and ancestral homes lost. The abrasive director of the TVA, S. David Freeman, tried to "pour cold water" on the dam and thus opposed Wagner and Senator Howard Baker, who formed, then dismissed a Congressional committee when it disagreed with him. According to the press, Baker thus drew on himself "the contempt of Congress and the nation." But Baker maneuvered legislation despite court action, and the Tellico Dam finally opened. Meanwhile, opponents had charged that a three-inch fish called "snail darter" in the river would be endangered, only to learn later that it still survives nearby. The controversy lingers. In the summer of 1986 a Tellico state office for attracting industry was burned, arson being suspected. The book is an interesting, mainly unbiased, account of a colorful episode in the history of the TVA.

NORTH CALLAHAN
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KENNETH FOX. *Metropolitan America: Urban Life and Urban Policy in the United States, 1940–1980.* Jackson: University Press of Mississippi. 1986. Pp. xiii, 274. $27.50.

In this book Kenneth Fox presents his interpretation of urban development and national policy during the period 1940–80. He claims that he has two purposes. First, he wishes to show "that official government data, social science research, and sound logical argument can shape and change government policy" (p. ix). Thus, he denies that policies are primarily the product of politics. Second, he argues that "good national policy" for cities depends on the coordination of government action with what he refers to as "forces of settlement," that is, patterns of migration and life style. He identifies two major migrations that transformed the United States after 1940: the movement of southern blacks to northern central cities and the exodus of working-class and white-collar families to the suburbs.

In his early chapters Fox focuses on the social and cultural consequences of suburban development. He argues that before World War II the American social structure consisted of a business class and a working class. In postwar suburbia, however, a new middle class took shape, and the earlier, bifurcated social structure was destroyed. In a wide-ranging analysis that includes references to C. Wright Mills, Ozzie and Harriet, and *Rebel Without a Cause*, Fox describes this emerging middle-class culture.

Later chapters examine the development of policy to save the central city, the riots of the 1960s, and the policy debate over the urban crisis. Fox rejects the Kerner Commission's contention that urban rioting was a black protest against racial oppression by whites. Instead, the riots were a positive assertion of power by blacks, a conscious attempt to gain recognition and clout. Dismissing politics as irrelevant, Fox views the conflict over policy on the urban crisis as a debate among social scientists who attempted to define the supposed crisis, identify its causes, and suggest solutions. According to Fox, this conflict burned itself out as experts were caught in the contradictions of their own arguments. Amid the shambles of the debate on the urban crisis, Richard Nixon's New Federalism arose and transformed national programs for dealing with the inner-city poor.

When the Democrats returned to the White House in 1977, they did not dismantle the community development programs of the Nixon administration. Instead, Jimmy Carter adopted a policy that Fox harshly condemns. The Carter White House officially resigned itself to the deterioration of urban life and thus gave "the people of older, declining central cities no basis for surviving the transition to postindustrial urbanism with hope, or even guarded enthusiasm" (p. 225).

Throughout his work Fox stresses ideas that some may regard as dubious. Many might question his approach to the determination of policy, and some might react strongly to his interpretation of the American social structure. His sometimes opaque prose does not make for easy reading. But those who persevere and finish this book will be well aware of Fox's decided views on the course of urban development and policy during the decades following World War II.

JON C. TEAFORD
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MICHAEL SCHALLER. *The American Occupation of Japan: The Origins of the Cold War in Asia.* New York: Oxford University Press. 1985. Pp. xii, 351. $22.50.

The epochal Allied occupation of Japan was a focus of interagency rivalry in Washington and a major cause of animosity between the United States and the Soviet Union. Michael Schaller's new study of

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the formulation of policy regarding postwar Japan is a welcome contribution to the literature on the evolution of the cold war in Asia. Although Schaller's monograph is not comprehensive, even on occupation policy and contains no major surprises, it is the best work so far on the personal conflicts and power struggles of the makers of U.S. policy on Asian affairs during the administration of Harry S Truman.

After a few sensationalistic opening pages the work is usually moderate in tone and reasoned in analysis. The reader's attention is held by smooth narrative that is skillfully seasoned with well-chosen quotations. Warm-blooded episodes such as George Kennan's audience with Douglas MacArthur in Tokyo contribute pathos and personal insight to the account. The author employs the traditional methodology and standard written sources of the diplomatic historian. Sources cited include U.S. documents (some ferreted out by the Freedom of Information Act), British manuscripts, memoirs, secondary works, and the Western press—but, surprisingly, no interviews. Key actors are the State Department, the Joint Chiefs of Staff (JCS), the Supreme Commander for the Allied Powers (SCAP), the China Lobby, Prime Minister Shigeru Yoshida, and President Truman—with ample staging of Kennan as an architect of cold war policy.

This treatment of the occupation of Japan is the work of an American diplomatic historian, not of an expert on Japan. The scene is usually Washington and its policy intrigues. Schaller devotes little attention to the Japanese experience of ingesting the occupation and, except for an extended treatment of *zaibatsu* deconcentration, does not tell how the policies of the JCS and SCAP fared once they were enunciated in Tokyo. Even on American occupation policy Schaller is selective in the issues he treats: Japanese reparations, rearmament, economic revitalization, and the peace treaty. Schaller's references to Yoshida do not advance our meager understanding of how the Japanese shaped their own occupation. Noteworthy is Schaller's analysis of Stalin's objection to the American insistence on a unitary occupation and the Soviet leader's canny seizing on this circumstance to vindicate Soviet sway in Eastern Europe and the Balkans.

Although he occasionally credits MacArthur with standing firm on such worthy principles as an early peace treaty and Japanese pacifism, Schaller generally paints an uncomplimentary picture of the supreme commander and his Tokyo staff. Frequently distracted by his presidential ambitions and hatred for the Democratic commanders-in-chief under whom he served, MacArthur was beholden to unsavory rightist power brokers. He resented directives and prodding from his superiors and took credit for reform policies that had been forged in Washington. The author depicts an arrogant and vacillating man whose influence steadily declined until "MacArthur's ideas were neither solicited nor heeded by the Truman Administration" (p. 71). By 1948 the State and Defense Departments saw the supreme commander as "the greatest single danger to Japan's security" (p. 121).

Schaller has consistently sought to demonstrate a close relationship between occupation policy and Southeast Asian policy. In earlier writings he has posited a determination on the part of Washington to restore trade between Japan and Southeast Asia both as a stimulus to the revival of the Japanese economy and as a means to lure Japan away from trade with the People's Republic of China. This theme is reiterated throughout the book. Although Schaller succeeds in showing the contextual similarity between developments in occupation and Southeast Asian policies, he falls short of proving that one vitally affected the other. In fact, in his extended discussions on Indochina in the late 1940s, Schaller seems to lose sight of the occupation. The same can be said of his detailed treatment of U.S. aid to the retreating Chinese Nationalists.

Revisionists will take little comfort in Schaller's clear espousal of the standard "reverse course" interpretation of the occupation. Focusing on reparations and deconcentration, he asserts that "an initial reform impulse fell victim to a conservative trend" (p. 39). This reorientation of policy, in Schaller's view, was a product not merely of changed Japanese circumstances or a fresh appreciation of Japanese economic and security needs but of a willful choice on the part of the State Department in pursuit of American self-interest. MacArthur, who initially resisted the conservative modification of his reform programs, acquiesced and moved to the right; the Korean War sealed the question of Japanese rearmament. Quotations to the effect notwithstanding, Schaller's repeated references to the reconstitution of Japan's prewar East Asian order are simplistic in view of the profound reform in attitudes effected by defeat in war and Japanese reeducation at the hands of SCAP.

On the whole, Schaller has provided a readable, credible, and scholarly diplomatic history of the occupation of Japan. The publication of his contribution provides occasion to call for a comprehensive treatment of the era of the occupation, one which synthesizes the prodigious Western and Japanese research of the past forty years and draws on the rich memories of both the occupiers and the occupied.

THOMAS W. BURKMAN
Old Dominion University

THOMAS J. NOER. *Cold War and Black Liberation: The United States and White Rule in Africa, 1948–1968.*

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Columbia: University of Missouri Press. 1985. Pp. xiii, 274. $26.00.

This is a detailed study of U.S. diplomacy toward South Africa, Rhodesia, Angola, Mozambique, and Namibia between 1948 and 1968 with an epilogue covering the period from 1968 to 1980. The book is well researched; in fact, Thomas J. Noer is one of the first scholars to have made extensive use of declassified materials on Africa in the National Archives and in presidential libraries. The author is very good on the role of lobbies in foreign affairs, on the struggles within the federal bureaucracy, and on the ignorance and misconceptions of U.S. officials about Africa. But he overestimates the importance of the division between "Africanists" and "globalists" in deciding government policies, and he is wrong in claiming that the globalists did not wish to alienate the white rulers of southern Africa and that they dominated policy decisions. Only Henry Kissinger tried to follow a globalist policy, as did the Reagan administration after 1980. The United States voted for most sanctions and boycotts against South Africa and would not let the U.S. Navy stop at any South African ports. The United States even supported the total boycott of arms against South Africa, hardly things a globalist would have supported.

Although Noer's documentation is solid, his interpretations are flawed by a moralistic, legalistic approach to foreign policy. He mistakenly thinks U.S. policy was much the same, that is, prowhite and anti-African nationalist, under each president. The author makes too much of American capabilities to have effected change. The United States was always anticolonial and antiapartheid. Every president spoke out against apartheid; even Richard Nixon attacked it as a threat to world peace, and Jimmy Carter and Ronald Reagan called for an end of apartheid. Carter even supported equal suffrage and took a civil-rights approach in the mistaken belief that South Africa was Georgia writ large.

No American administration has had much real leverage over Portuguese, Rhodesian, or South African settlers. Soviet involvement in Africa, moreover, was not caused by U.S. involvement. The Soviets have always had their own agenda. When the United States tried to keep the cold war out of Africa after 1960, the Soviets ignored Washington and continued to support political fronts under Marxist-Leninist domination, such as the African National Congress, the Popular Movement for the Liberation of Angola, and the South-West African Peoples Organization. When Carter stopped selling arms to Africa (disarmed diplomacy), the Soviets increased their arms sales. The Soviets thrived on the conflict in southern Africa, which the United States could not control.

Noer's conclusion is, in my view, too pessimistic. Major changes in South Africa can be achieved without civil war because the white rulers, though militarily strong, appear willing to abolish apartheid, albeit at their pace. If U.S. policy demands majoritarian rule, as Noer recommends, a bloody civil war could result.

The bibliography is full but flawed. Noer does not list books that give a different interpretation of U.S. policy toward South Africa. He is also weak on books that treat Soviet activity in Africa. Still, in spite of its moralistic approach, this book is a solid piece of work.

PETER DUIGNAN
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GEORGE E. SIMS. *The Little Man's Big Friend: James E. Folsom in Alabama Politics, 1946–1958*. University: University of Alabama Press. 1985. Pp. x, 271. $29.50.

CARL GRAFTON and ANNE PERMALOFF. *Big Mules and Branchheads: James E. Folsom and Political Power in Alabama*. Athens: University of Georgia Press. 1985. Pp. xiv, 307. $27.50.

James E. Folsom was twice governor of Alabama during the 1940s and 1950s. He was an unusual person. In a land of cheap labor, he was an ally of the CIO. In a land dominated by Dixiecrats, Folsom supported Henry A. Wallace. In a land of Southern Baptists, he struggled with paternity suits and alcoholism. In a land of segregation, he sought to expand civil rights and consistently went out of his way to recognize the dignity of black Alabamians. In a land of disfranchisement, he endeavored to place blacks and poorer whites on the registration rolls. Yet, Folsom was for more than a decade the most popular politician in Alabama.

These books by Carl Grafton and Anne Permaloff and by George E. Sims are the first full-scale studies of Folsom and, except for William D. Barnard's *Dixiecrats and Democrats* (1974), are the only substantial published accounts of the controversial governor. Both books are quite good. Both place Kissin' Jim (who liked to buss the ladies who attended his campaign rallies) within the broader perspective of Alabama and southern politics. Both trace his liberal lineage back to populism. Both lament his self-indulgence, and both agree that by the middle of his second term Folsom had become an alcoholic. Both fault the governor for lax administration and criticize his tendency to favor subordinates who were loyal above those who were only competent or honest. Both also document Folsom's talents as a politician who was an innovative campaigner and who, on occasion, was capable of dealing with a generally hostile legislature with remarkable adroit-

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ness. Both conclude that Folsom was a flawed hero who failed to accomplish his goals. Yet, despite broad areas of agreement, the books differ significantly in their interpretations.

Grafton and Permaloff approach their analysis from a power-elite perspective. In Alabama an "elite alliance" of black-belt planters and Birmingham Big Mule industrialists structured a political system based on a malapportioned legislature, a truncated electorate, and the constitution of 1901. Despite factionalism in the state Democratic party, it remained "a system that confined overt decision making to routine matters during the 1940s and 1950s" (p. 254). Folsom represented a deliberate and ideologically radical attack on the system. "The political system was corrupt," Grafton and Permaloff write, "and Folsom's basic goal was to end that corruption" (p. 98). Thus, the Folsom era witnessed an intense struggle between an entrenched "elite alliance" and a popular insurgent movement led by a charismatic mass leader.

In his analysis of the Folsom years Sims uses modernization theory to depict the tensions "that accompanied the thrusting of a locally oriented society into the confusion of America's mass culture" (p. 5). Sims agrees with Grafton and Permaloff that the alliance of cotton and steel dominated Alabama politics, but in Sims's interpretation the planter–Big Mule coalition was defending localism against the centralizing forces unleashed by the New Deal, World War II, and other developments. From this perspective Folsom appears to have been neither particularly radical nor ideologically oriented. In Sims's words, "Big Jim did not seek a fundamental shift of political power or a revolution in social mores" (p. 163). Instead, he emerges as a transitional figure: "Jim Folsom's actions, proposals, and attitudes belonged more to the emerging mass society than to the isolated, locally oriented world that was disappearing" (p. 226). Rather than an insurgent mass leader, Folsom was something of a harbinger of the sunbelt South.

Perhaps as a result of these varying perspectives, the two books treat aspects of Folsom's governorships quite differently. Sims emphasizes Folsom's personal failings; his text is laced with descriptive phrases identifying Folsom with "incredible carelessness" (p. 43), "cynicism" (p. 45), "embarrassing personal conduct" (p. 79), and "flaws within Folsom's character" (p. 190). Grafton and Permaloff stress the entrenched power of Folsom's opposition and the undemocratic and "corrupt" nature of the political system Folsom sought to change. Sims is concerned with Folsom's personal morality and is unsympathetic with Folsom's efforts to undermine the state merit system in Alabama's modernizing bureaucracy and with his use of bribery and patronage to influence the behavior of the state legislature. Grafton and Permaloff imply that it was not entirely irrational for Folsom to attempt to gain control of his administrations by revamping the bureaucracy in his attack on the system. Although Grafton and Permaloff do not condone Folsom's more unsavory public practices, they do demonstrate the dilemmas faced by a radical reformer. Lacking access to the awards normally bestowed on tame politicians (law-firm retainers, good business relations or employment opportunities with large corporations, and so on), Folsom and his supporters may have had little choice but to either betray their campaign promises or to use the available sources of influence, including bribery and blatant patronage. Similarly, Sims comments on Folsom's "inept handling of the news media" (p. 50); Grafton and Permaloff suggest that Folsom's program ensured opposition from the urban press no matter how adeptly he dealt with the media. Such examples could be extended, and they could also be exaggerated. Except for occasional minor detail, the two books are in factual agreement, and they tend to complement each other more than to conflict.

In an excellent concluding chapter Grafton and Permaloff examine per-capita state expenditures for various public programs during Folsom's administrations as compared with the terms of other Alabama governors, with other southern states, and with the nation. They conclude that "public policy in Alabama would probably have been little different had James Folsom never been elected governor" (p. 253). Sims's conclusions are similar. In the end, according to both books, the sound, fury, and conflict surrounding the Folsom years signified little. Surely there is more to it than that.

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GERARD T. RICE. *The Bold Experiment: JFK's Peace Corps*. Notre Dame, Ind.: University of Notre Dame Press. 1985. Pp. xv, 349. $15.95.

If the New Frontier today no longer evokes excitement among many historians, one aspect of that program—the Peace Corps—has retained some luster. The reason, as Gerard T. Rice reminds us, is that it was "an early, dramatic step in the direction of global cooperation" (p. 302).

Rice's first-rate history traces the Peace Corps from the pioneering ventures of private organizations through proposals by Henry Reuss and Hubert Humphrey for a government program and a campaign promise made by John F. Kennedy in 1960 to its implementation under the dynamic direction of Sargent Shriver. Although Republican leaders ridiculed the idea and Congress, disturbed by the prospect of "radical" and "beatnik" volun-

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teers, cut its proposed funding, the new agency was soon flourishing. Popular support, inventive leadership, and genuine need carried it past all obstacles. By 1964 the Peace Corps had ten thousand volunteers working in forty-four countries. Contrary to the media image, most volunteers served as teachers and lived in modest comfort. Rice shows convincingly that they had a beneficial effect on Third World societies and that these societies had an important impact on the volunteers.

Overall, this is a fine scholarly study. Rice draws on extensive Peace Corps records, interviews, and personal manuscript collections. The book's organization is excellent, the writing mellifluous.

Unfortunately, the analysis is not always very penetrating. Although Rice discusses economic development, he never seriously confronts structural barriers to economic growth and social equality. Furthermore, he seems overcome by the Kennedy mystique. Thus, the volunteers unaccountably emerge as "Kennedy's children" (p. 303), despite substantial evidence of the president's disinterest. Explaining Kennedy's relative neglect, a White House aide commented that the Peace Corps "was nice but it just wasn't that important" (p. 138).

This book clearly shows the extraordinary appeal the Peace Corps had for young people and ordinary citizens. By the end of 1960 Kennedy had received more than twenty-five thousand letters about the program—more letters than on any other subject. A survey in 1961 found that 94 percent of college students supported the idea. To many, the Peace Corps seemed a creative alternative to cold war policies. Asked why they had joined, volunteers responded most frequently that they sought "to help people and humanity in general" and least often that they hoped "to fight communism" (p. 168). The Peace Corps, in short, tapped the desire of many Americans to play a new, more constructive global role. As the author concludes, in contrast to those who "led America into disastrous war in the Third World," Peace Corps volunteers constituted "the real best and brightest on the New Frontier" (p. 302).

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CANADA

CRAIG HERON and ROBERT STOREY, editors. *On the Job: Confronting the Labour Process in Canada.* Kingston, Ont.: McGill-Queen's University Press. 1986. Pp. xiv, 360. Cloth $37.50, paper $16.95.

Throughout the past decade historians and sociologists have been revising our perceptions of labor history in Canada at an astonishing pace. Without pretending to be comprehensive, this volume of twelve essays indicates much about the strength and weakness of the new labor history movement in Canada.

The collection is eclectic; topics range from railway work in the 1850s to reflections on personal employment experience in the 1980s. Four of the papers address the experience of women (in offices, clothing manufacture, fast-food restaurants, and at home); two examine labor in resource extraction (coal mining and logging); two look at heavy industry (auto and steel); one paper treats the transition from handicraft to mechanized production (printing); and another examines the unique position of longshoremen. Some of the papers draw on research that has been reported elsewhere, whereas others make fundamentally new contributions.

Although each paper examines some aspect of the "labor process," the content of this concept is elusive. In their introduction Craig Heron and Robert Storey acknowledge being defeated by the very diversity of experience to which their volume is testimony: "the problem of constructing a theory of the labour process is that there is no one single labour process within capitalist society" (p. 32).

The papers vary in form as much as in content. The article by Paul Craven and Tom Traves on early railway work and Ian Radforth's treatment of pulp loggers are finely crafted pieces of historical writing; the volume deserves to be examined for these two papers alone. By contrast, other pieces tend more to journalism, and some, regrettably, lapse into jargon.

The strength of the volume lies in the willingness of its authors to draw on social theories and to think critically about labor's experience of historical change. Also, the sheer energy and diversity of the new labor history movement is reflected in this volume. The papers nevertheless hint at certain problems. As the editors acknowledge, labor historians require a working knowledge of market trends and the technological forces affecting employers. Unfortunately, Canadian economic and technological historians have not yet produced a sufficient number of detailed and reliable background studies. Not surprisingly, the strongest papers, such as those by Radforth and by Craven and Traves, make legitimate contributions to economic and technological history as well as to labor history.

Other papers, unfortunately, are based on a mechanistic and in places erroneous account of the technical and economic environment. The article on steelworkers, for example, contains several howlers: that steel companies in Nova Scotia encountered difficulty only after a corporate reshuffling in 1920, that all pricing decisions in the Canadian steel market were controlled by three firms, and that techni-

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cal progress was primarily intended to reduce labor rather than material and inventory costs. Moreover, the dates assigned to important innovations in steel technology are incorrect; possibly as a consequence, the authors, Heron and Storey, fail to appreciate the extent to which Canadian firms adopted equipment that was in some respects obsolete. In spite of these limitations, the paper lucidly and engagingly summarizes much of the literature relating to Canadian steelworkers.

The importance of economic context is illustrated again by David Frank's discussion of coal miners in eastern Canada during the 1920s. Labor's informal and formal struggle for control of the labor process is described with no apparent recognition of the limited rewards available to be shared with capital. As a result the author cannot fully assess the extent of labor's success or failure; data on earnings are not provided much less related to the cost of living, the price of coal, or company profits. A failure to appreciate the mariginality of the eastern Canadian coal field during the 1920s may contribute to a second questionable interpretation. The author portrays the coal miners' advocacy of public ownership of the mines as a natural expression of the workers' struggle for control rather than a more pedestrian grasping at the straw of public subsidy. Frank nevertheless provides a fascinating and all-too-brief account of the technological backwardness of coal mining in eastern Canada and indicates some of the implications for labor.

A second serious weakness of the new labor history is illustrated by the paper on the steel industry, which undertakes a conceptually sophisticated analysis on the basis of somewhat frail evidence. A major conclusion of the paper, for example, is that job structures in the Canadian steel industry evolved in a way that parallels the American steel industry (as described by David Brody and Katherine Stone). Although it would be surprising if this were not true, the authors provide remarkably little firm evidence.

The volume exhibits a tendency to assert or assume points that might better be supported by evidence. One unhappy consequence may be that the potential audience for the volume is unduly narrowed; several papers will hold little interest for those who do not share the presuppositions and world view of the authors.

The volume is most valuable for individual papers and as a representative sample of the new labor history movement in Canada.

KRIS INWOOD
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D. J. HALL. *Clifford Sifton.* Volume 2, *A Lonely Eminence, 1901–1929.* Vancouver: University of British Columbia Press. 1985. Pp. x, 437. $26.95.

Over half of the concluding volume of D. J. Hall's life of Clifford Sifton is concerned with Sifton's five final years as a member of Sir Wilfrid Laurier's cabinet. Sifton was a powerful man; observers often assumed that he dominated the government. He was the political boss of western Canada, virtually the dictator of the Yukon, and without question one of the most able administrators in Laurier's government. Yet, during these final years in the cabinet, Sifton's power failed to fulfill his desires. His administration of the Yukon could by no means be called a success; the Alaska boundary adjudication was essentially a victory for the Americans; he failed to control federal transportation policy; his post as minister of the interior lost its challenge; and he became increasingly frustrated with his place in public life. Finally, in 1905, Sifton left the government. His resignation and the attendant circumstances constitute one of the murkier passages in modern Canadian politics. Hall does a particularly effective job of analyzing this incident. Sifton's objections to the education clauses in the autonomy bills are clearly explained as is the almost unbelievable deviousness of Laurier. Hall argues convincingly that the differences between the two men were matters of substance and principle. Although Sifton won, the costs were high: Sifton never again served as a member of the government. Why Laurier never brought Sifton back is explained but not definitively. The ultimate reason may well be that Laurier regarded Sifton as a political liability by 1905. His reputation was that of a crooked machine politician who went into government poor and emerged rich. Charge after charge was leveled against Sifton. They were never proved, but neither were they ever shaken. As Hall notes, even the governor general regarded Sifton as "a blackguard and a villain" (p. 1).

After 1905 Sifton exercised substantial influence on several occasions. In 1911, for example, he defected from the Liberal party when Laurier opted for a reciprocity agreement with the United States. He was one of those working with R. L. Borden, the Tory chief, to force an election and then defeat the Liberals and reciprocity. All the while his newspaper, the Manitoba *Free Press*, campaigned for reciprocity. Hall explains this anomalous situation: the editor of the *Free Press* convinced Sifton that the newspaper would be damaged commercially if it failed to support free trade. Again in 1917 Sifton played an influential role when he worked to get conscriptionist Liberals to break with Laurier and enter a coalition government under Borden.

The Sifton that emerges is as unattractive as he was in volume 1. He was dour, extremely ignorant of French Canada, greedy, nasty, and authoritarian. Sifton was a constant political manipulator, and Hall's material on electoral management is some of

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his best work. Sir Clifford, as he was after 1915, does not seem to have had a sense of humor, although he could be unintentionally funny—as when he described Sir George Foster as "a useless, garrulous, crooked old humbug" (p. 325).

As is well explained in volume 1 of the biography, Sifton played a major role in shaping the modern prairie region and is therefore an important figure in Canadian history. Hall has supplied a thoroughly researched, sensibly organized, well-written, and cogently argued biography.

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SERGE GAGNON. *Quebec and Its Historians: The Twentieth Century*. Translated by JANE BRIERLY. Montreal: Harvest House. 1985. Pp. 205. $12.50.

This book contains five essays on French-Canadian historiography since the 1940s, of which three have been previously published in French. Serge Gagnon's main concerns are the influence of historians' social environment in shaping their scholarly work and the opposing influence of method in protecting against total relativity.

The first three essays trace the evolution of Quebec's historical scholarship during the postwar era and period of "quiet revolution." Here Gagnon is at his best, explaining historical schools and involved theories in a clear and straightforward manner that makes them perfectly accessible to students and nonspecialists. He relates changes in historical interpretation to social changes—secularization, the emergence of new classes and new ideologies—and to the introduction into Quebec of new scholarly techniques, first from the United States and later from Europe. With balance, lucidity, and good sense he analyzes the tension between bias and technique, ideology and method. His demonstration that society and ideology influence even the most professional modern historians, just as they did the earlier clerical scholars, is readable, clear, and compelling. It might have been discouraging too, were it not for the equally clear demonstration that new scientific techniques and professionalism have brought more reliability and intellectual conviction.

The rest of the book is devoted to one historian, Fernand Ouellet. Since Ouellet bestrides the narrow world of Canadian history like a colossus, an evaluation of his work is certainly welcome. For years, in fact, scholars have quietly grumbled that this greatest Canadian apostle of "scientific" history showed many unscientific traits: strong moral judgments, absence of footnotes or precise evidence to support some affirmations, and uncritical use of certain sources. Gagnon presents these and other criticisms at great length.

But Gagnon's treatment of Ouellet lacks the balance of his other essays. Indeed, he attacks the great man with a startling ferocity. It is as if he were not content to show that Ouellet, like others, is influenced by ideology; he depicts Ouellet as a man almost completely blinded by bias. Ouellet's scholarship is presented as if it were a sham, a façade, hiding "a subjective interpretation" (p. 138), "wishful thinking" (p. 83), and "emotional reaction" (p. 108). Gagnon does not merely assert that Ouellet's work, like everyone else's, is subject to amendment, reinterpretation, and revision; he exposes every flaw, catches, defines, and magnifies every questionable affirmation, and opposes every hypothesis or interpretation with another. In short, Gagnon goes too far, expressing his criticism in exaggerated terms, belaboring petty points, and using questionable evidence himself while accusing Ouellet of doing the same. He does not apply to Ouellet the same criteria he uses in discussing other historians. His criticism would have been more convincing if it had been more moderate.

Clearly, then, this book lacks unity. The first half is addressed to a general readership and serves it well; the second half is for specialists familiar with Ouellet's work and is bound to provoke them considerably.

A. I. SILVER
University of Toronto

LATIN AMERICA

LYLE N. MCALISTER. *Spain and Portugal in the New World, 1492–1700*. (Europe and the World in the Age of Expansion, number 3.) Minneapolis: University of Minnesota Press. 1984. Pp. xxvi, 585. Cloth $35.00, paper $13.95.

In recent years it has become the practice for works of synthesis in the field of early Latin American history to include both the Spanish and the Portuguese spheres within a common framework, and the present publication by Lyle N. McAlister is no exception. It will occupy a niche of its own, presenting the most detailed and comprehensive treatment of the first two-thirds of the preindependence period available in any unified book in English. Although potentially useful as a text and perhaps partially intended as such, the book's approach is notably more historiographical than is usual in that genre. The names of individual modern scholars are scattered throughout the body of the volume, and its pages are quite closely footnoted. In addition, whole historiographical controversies are judiciously re-

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viewed, including questions such as the validity of the notion of a seventeenth-century depression, ethnic relations, and dependency theories. A detailed, lucid resumé of the standard historiography of the Iberian background is perhaps the most striking single feature of the volume.

Using laws and official reports as a window onto social and economic life, some of McAlister's most illustrious earlier work operated at the juncture of government and society; for all its breadth, the new book carries much of the same emphasis. The crown and governmental-ecclesiastical institutions are at the forefront and serve as the prism for the rest. Macroeconomic and macrodemographic developments also receive serious consideration, insofar as serial institutional data can throw light on them. Less emphasized, and often entirely missing, are direct description and analysis of social and economic structures, whether family, careers, the shape of estates and commercial organizations, or the organization of labor. The history of Indians hardly figures at all, and what is said on that topic represents perhaps the only aspect of the volume that is thoroughly outdated at publication. In short, the author is strong on all the subdisciplines that were being practiced about a generation ago and has kept current with even the most recent developments in those subfields but shows much less interest in some of the branches coming to prominence in the last fifteen years or so.

Brazil, sometimes quite swallowed up in Iberoamerican histories, receives its due, although Spanish dominance of the western hemisphere in the first half century inevitably sets the tone for the book. Correctly discounting any inherent differences between Spaniards and Portuguese, McAlister seeks instead to base comparisons on specific conditions in both hemispheres, and the actual comparisons and contrasts that he draws are well founded. He follows, however, the older method of taking Brazil as one unit and all of Spanish America—dominated by Mexico and Peru—as another. The result is to present as Spanish-Portuguese many distinctions that actually should be made between Mexico-Peru and all the rest of Iberoamerica, whether Portuguese or Spanish.

Although the reader will need to consult other items in the growing literature of historical synthesis on colonial Latin America for supplementary perspectives and information, McAlister's book provides many facts and thoughts not available outside specialized monographs. This rich and solid volume should be useful for many years to come.

JAMES LOCKHART
University of California, Los Angeles

MANUEL MORENO FRAGINALS, editor. *Africa in Latin America: Essays on History, Culture, and Socialization.* Translated by LEONOR BLUM. New York: Holmes and Meier or UNESCO, Paris. 1984. Pp. 342. $45.00.

This delightfully readable and accurate translation of the Spanish-language volume that first appeared in 1977 is a welcome addition to the enormous literature on the Afro-American aspects of Latin American and Caribbean societies. It is not identical to the previous volume, however. The essays by Edward Brathwaite, "The African Presence in Caribbean Literature," and Sidney Mintz, "Africa of Latin America: An Unguarded Reflection," have been restored to their original forms with abundant, informative footnotes. But, as if to offset this, three of the original selections have been dropped without explanation and some of the remaining essays have been edited—although the only significant deletion appears to be the musical score for the Children's Round (Tucumán) in Isabel Aretz's essay, "Music and Dance in Continental Latin America, with the Exception of Brazil" (p. 224). Missing are the chapters by Jean Benoist on the social organization of the Antilles, by Adelaida de Juan on the plastic arts in the Antilles, Mexico, and Central America, and by Nitza Villapoll on African dietary customs in Latin America.

Unlike the original, the fourteen essays are grouped under three subdivisions: "Historical Beginnings: From Plantation Life to Class Structure," "Cultural Forms: Religion, Literature, and Music," and "Socialization and Development." All the essayists try to cover as much of the region as is practicable, and together they make reference to nearly every Latin American and Caribbean country. A slight ideological assumption creeps through some works but is insufficient to detract from their exacting scholarship and insightful thought.

In the first section Manuel Moreno Fraginals provides a survey of five centuries of the multifaceted African legacy, with particular emphasis on cultural and demographic aspects. Germán Carrera Damas differentiates between "the formal existence of slavery [and] its real existence" (p. 24) and concludes that the "absolute destruction of slavery [was] not possible within the structure of a capitalist society" (p. 37). Octavio Ianni discusses the subtle ways in which social organizations have alienated Afro-Americans both racially and by class.

The most stimulating essays are in the section dealing with the impact of African culture on American languages, music, literatures, and religion. Richard Allsopp's highly technical examination persuasively demonstrates that in all linguistic cases, but especially in the English and French Caribbean, this influence, rather than being superficial, constitutes a

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fundamental alteration in vocabulary, syntax, intonation, and idiom. Brathwaite and Samuel Feijoo conclude that in poetry and prose, "the influence of the words, euphonies, myths, proverbs, rhythms, dances, and songs, of the tense personality of the Black African, inherited and transformed by the descendants of those who had been slaves, is a creative force in present-day Latin America" (p. 169). In covering the musical panorama of the entire region, Aretz, Odilio Urfé, and José Jorge de Carvalho illustrate the various secular and religious modes that facilitated the development and transformation of musical styles and tastes.

Two of the four final essays convey strong political overtones. René Depestre, with more rhetoric than research, asserts that the inevitable universal victory of socialism will make the present dichotomy of class and race anachronistic. In a thoughtful piece Mintz reminds the reader of some of the neglected aspects of Afro-American history and cautions that the relationship between Afro-American development and the rise of capitalism and the linkages between African and Afro-American culture are "less important scientifically than the study of the particular integrations and symbolic forms developed by New World peoples in the consolidation of new societies" (p. 299). Indeed, the focus on culture and society sets this book apart and commends it to specialists.

FRANKLIN W. KNIGHT
Johns Hopkins University

DAVID BARRY GASPAR. *Bondmen and Rebels: A Study of Master-Slave Relations in Antigua; with Implications for Colonial British America*. (Johns Hopkins Studies in Atlantic History and Culture.) Baltimore: Johns Hopkins University Press. 1985. Pp. xx, 338. $35.00.

This meticulously researched and well-written book is an extremely useful addition to the growing body of literature on slavery in the British Caribbean. David Barry Gaspar uses conspiracy on Antigua in 1736 to provide the most comprehensive treatment to date on slave resistance in a British Caribbean territory. Realizing that slave resistance can best be studied in conjunction with slave control, Gaspar sees this conspiracy as "a window into Antigua slave society to explore questions that are also of relevance to other New World societies" (p. xiv).

The book is divided into three parts. In the first portion the author discusses the specifics of the conspiracy and its consequences. The "crisis mentality" that whites developed following the disclosure of the plot led to the passage of draconian legislation that significantly limited the activity of slaves and simultaneously placed extraordinary power in the hands of the local magistracy. Indeed, had it not been for the intervention of officials in Britain, local authorities would have effectively changed the laws so as to use the testimony of slaves to indict free blacks yet deny its use against whites. The case of the Johnson brothers, which Gaspar chronicles, demonstrates the adverse effects this novel legislation would have had on the free black population at this critical time.

Part 2 presents a larger picture of the dynamics within slave society. The increasing recruitment of large numbers of African slaves and consequent absolute and proportional diminution of the white population necessitated the passage of laws aimed at regulating the behavior and actions of slaves. Funerals of slaves came under the view of white officials, and the Comprehensive Slave Law of 1697 represented a major attempt by whites to control systematically slave life and conduct. That this and subsequent laws dealt increasingly with the thorny question of runaways suggests that slaves were becoming more restive.

Gaspar elaborates on this theme in part 3, which describes the adverse economic conditions that apparently contributed to continual and increased flight within and outside the island during the early 1700s. Masters were "shocked" to discover that their "trusted" slaves were involved in a conspiracy of such far-reaching implications. The conspirators, who had never really liked their masters, came not from among the ranks of the field hands but mostly from "privileged" Coromantee and Creole slaves who had forged an alliance aimed at gaining control of the entire island.

Gaspar speculates briefly on what the nature of society would have been had this conspiracy—hatched a full fifty years before the successful uprising in Haiti—succeeded. Whether the Coromantee-Creole alliance would have lasted or whether other ethnic groups would have been kept in bondage are both intriguing questions. The fierce cultural resistance exhibited by Antigua's slaves in 1736 requires the reevaluation of a number of assumptions that are often made about the relative absence of African survivals in some slave societies.

This excellent study by a brilliant young scholar provides useful new dimensions for looking at the complexities of New World slave societies. Students of slave resistance must now contrive to search much earlier into the eighteenth century for examples—and the attendanct implications—of massive uprisings or conspiracies of slaves.

EDWARD L. COX
University of South Carolina

REBECCA J. SCOTT. *Slave Emancipation in Cuba: The Transition to Free Labor, 1860–1899*. Princeton: Princeton University Press. 1986. Pp. xviii, 319. Cloth $44.00, paper $13.95.

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In the past decade the historiography of slavery has been moving from a structuralist approach, concerned with the long-term forces shaping people's lives and setting limits to their choices, to a phenomenological one, examining the choices they made and the ways those choices affected their lives. Historians now seem less interested in studying slavery as a system than as an experience. In the study of the transition from slavery to free labor, their attention has moved away from economic and social structures to relations between masters and slaves and the impact of institutional, political, and ideological changes on those relations.

As a consequence of this shift in perspective, historians have uncovered new sources and have questioned traditional interpretations. This change in perspective sometimes helps refine our understanding of the past, as Rebecca J. Scott's book illustrates. She challenges the traditional view—particularly as expressed in the work of Moreno Frajinals—that growing, insoluble contradictions gradually rendered slavery an irrational and unworkable institution. Using a variety of documents gathered primarily at the Archivo Histórico Nacional and the Biblioteca de la Real Academia de Historia in Madrid and at the Archivo Nacional de Cuba, Scott argues that, on the eve of the Moret Law, slavery was still a viable institution and that slavery and the use of machinery were not incompatible. Slaves tended to be concentrated precisely on the sugar cane plantations, where the degree of mechanization was highest, and most sugar planters opposed emancipation until the final years of slavery. They solved the problem of the relative inelasticity of the slave labor force by resorting to rented slaves and supplementary immigrant labor. Thus, nothing was compelling them to abandon slavery.

Scott stresses the political and institutional aspects of Cuban emancipation. In her opinion, the abolition of slavery in Cuba can be understood only within the context of Cuba's colonial situation. The abolition of the slave trade by the Spanish government, the Ten Years' War, the Moret Law, and, most of all, slaves' resistance and the support they received from individuals who favored emancipation—all these circumstances contributed in the long run to undermining the institution. After emancipation the state tried to mediate between capital and labor and to secure a labor supply but made no effort to influence the integration of former slaves into the new society. The state's role changed from securing labor discipline to guaranteeing labor contracts. Freed slaves found their way into Cuban society blocked by the influx of immigrants, the monopoly of land by plantation owners, and by the persistence of racial and ethnic barriers. As bondage had assumed a variety of different forms, so the range of postemancipation experience was equally multifaceted. Still, the province most committed to sugar and to slave labor became the area of most intense proletarianization. This may be enough to make us wonder whether, from a structural point of view, Frajinals was not correct after all when he argued that capitalism and slavery were fundamentally incompatible; from a conjunctural perspective, however, the transition from slavery to free labor did, no doubt, depend on the kinds of actions that Scott describes so well.

EMILIA VIOTTI DA COSTA
Yale University

CARMEN CASTAÑEDA. *La Educación en Guadalajara durante la Colonia, 1552–1821.* México City: El Colegio de Mexico, for El Colegio de Jalisco, Guadalajara, Mexico. (1984) Pp. 513.

In colonial Spanish America education was perceived as an activity for a small segment of the population. A means to achieve personal advancement for the individual, education was also assumed to reflect positively on family and community. Thus, although education remained a status symbol for those who were fortunate enough to receive it, the fostering of education was an honorable civic task. Despite its potential as a subject for the study of colonial social stratification policies, education remains a neglected subject in colonial historiography. This sweeping history of three hundred years of education in one of the most important geographical areas of New Spain does not change our understanding of its meaning but adds substantially to our knowledge of how the system functioned. Carmen Castañeda has carried out extensive research in Mexican and Spanish archives and has unearthed a significant measure of new information for what was basically intended to be a work of synthesis.

The study has two parts: the first one is a chronological history of all of the educational institutions of Guadalajara from 1552 through 1821. Here, Castañeda delineates the foundations of a variety of institutions, such as primary schools for boys and girls, special religious schools for girls, seminaries for the secular clergy, and the university, created by royal decree in 1791. In an effort to flesh out the institutional character of her story, in the second part she turns to the methods of instruction, the instructors, and the students. Castañeda meticulously describes the internal regimen of the schools and their objectives and drafts a prosopographical profile of the students, which, in the case of the university, includes data on intellectual performance. The chapter devoted to the University of Guadalajara is especially rich in data. Tables, graphs, and maps help convey more efficiently the vast and carefully gathered information.

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This is narrative history at its best: systematic arrangement of materials, solid research foundations, straightforward exposition of information. Castañeda's study lacks, however, any attempt to raise or answer teleological questions about the meaning of education as a tool for social control and as a reflection of cultural and moral values in a given historical period. Following traditional and safe historical paths, the author refrains from committing herself to any value judgments and delivers instead a well-drafted portrait that will be useful to those who need details before posing any questions on meanings.

ASUNCIÓN LAVRIN
Howard University

JAN BAZANT. *Antonio Haro y Tamáriz y sus aventuras políticas, 1811–1869*. Mexico City: El Colegio de México. 1985. Pp. 200.

Jan Bazant's excellent biography of Antonio Haro y Tamáriz is an important contribution to nineteenth-century Mexican historiography. Haro y Tamáriz was a member of one of Puebla's most prominent families. During the colonial period his relatives served the crown and the church and distinguished themselves in the economy of Puebla. After independence the family contributed to the economic development of the region, through haciendas, a textile factory, and cotton and grain mills.

As described by Bazant, Haro y Tamáriz was a complex man. He initially entered politics as a representative of the Puebla cotton interests. But, as he discovered a flair for politics, his horizons broadened, and he rapidly acquired notoriety in national politics. A strong supporter of Antonio López de Santa Anna, Haro y Tamáriz served as as minister of the treasury on three occasions, in 1844, 1846, and 1853. As minister, Haro y Tamáriz took a number of principled but unpopular stands that eventually forced him out of office. On two occasions he recommended the disentailment of church property; he indignantly rejected a proposal to establish the country's first bank because it was not in the nation's best interests. These actions alienated not only members of his class but even his own family.

Although widely regarded as a dandy, Haro y Tamáriz could also be a man of action. Unlike many aristocrats, he fought against the invading Americans and surprised his friends by his gallantry. Later, disillusioned by Santa Anna, he became the leader of the liberal revolt in San Luis Potosí. In 1856 he led an unsuccessful conservative revolt against Ignacio Comonfort and the Reform. When Comonfort triumphed, Haro y Tamáriz was forced into exile where he lived in poverty first in Europe and then in Cuba. Thereafter, he remained a pathetic figure until his death in Italy as a Jesuit noviciate.

Bazant, the author of many important economic studies of the period, has carefully reconstructed the public life of Haro y Tamáriz. He places him in historical perspective, demonstrating that, although he played a significant role, Haro y Tamáriz was not as important a figure as previously believed. Yet the question remains, why did his contemporaries consider him so important? Why did he achieve such prominence in national politics? Why did the people of San Luis Potosí and, later, Puebla recognize him as their leader in a struggle for national power? Although Bazant attempts to answer these and other questions about Haro y Tamáriz, he admits that he can give no satisfactory answers for now. The Haro family archive has few documents about Antonio, and current knowledge does not permit a full explanation.

The first half of the nineteenth century remains a puzzling period in Mexican history. Using Haro y Tamáriz as a vehicle, Bazant enlightens us not only about the life of this significant figure but also about the country he served. In addition, the author has raised a number of important questions that, I hope, he and other scholars will answer in the near future.

JAIME E. RODRÍGUEZ
University of California, Irvine

MOISÉS GONZÁLEZ NAVARRO. *La Pobreza en México*. Mexico City: El Colegio de México, 1985.

Poverty has been much studied by European and U.S. historians in recent years but not, ironically, in underdeveloped countries where poverty is so widespread. Now the eminent Mexican historian, Moisés González Navarro, has addressed the subject in a book written less for other historians than for the educated public, which is turning its back on the poor in the midst of an economic crisis of staggering dimensions.

This massive book ranges from the late eighteenth century to the present and touches on such disparate topics as Rotary clubs and liberation theology. Its focus is not actually poverty, as the title suggests, but the policies of church and state toward the poor. It traces the change from Catholic charity to public assistance and presents a wealth of information on beneficent institutions and government programs. Based on a wide variety of printed sources, especially newspapers and government reports, the book covers all regions of Mexico over two centuries, although it is strongest for the federal district and the twentieth century.

The point of this historical exercise is to show that poverty is a longstanding problem in Mexico, and one that persists despite recurrent efforts to solve

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it—including a revolution fought in the name of social justice. González Navarro believes that some approaches are better than others. Private charity fails because it is disorganized and piecemeal, because it humiliates the beneficiary, and because it merely treats the symptoms and not the causes of poverty. Government assistance is best because it is based on social solidarity rather than paternalism, because it attempts to change the structures that create poverty, and because it is national in scope and thus reaches rural areas untouched by centuries of philanthropy. González Navarro's desire to arouse the conscience of his compatriots is evident throughout the book, which moves from a first chapter entitled "What Shall We Do with the Poor?" to a last chapter entitled "What Shall We Do with the Rich?" The conclusion speaks to the rich when it argues that "preventing poverty is more charitable than trying to remedy it with alms" and "cooperating with state initiatives is the best way to perform charity" (pp. 450–51).

This books does not make for easy reading. The masses of detail often obscure the argument, and the chapter titles and subtitles sometimes add to the confusion, since they do not necessarily correspond to the contents. (For example, "The End of the Hacienda" opens with a fascinating discussion of Catholic Action, moves on to the founding of the Ministry of Public Assistance under President Lázaro Cárdenas, details various relief efforts during the Great Depression, and ends with a short analysis of popular movies of the 1930s; it does not, however, discuss the end of the hacienda.) Still, the patient researcher interested in exploring these subjects systematically will find many useful tidbits here, and the student of intellectual trends will want to follow the response to this book among educated Mexicans.

SILVIA M. ARROM
Indiana University

J. C. CAMBRANES. *Coffee and Peasants: The Origins of the Modern Plantation Economy in Guatemala, 1853–1897*. South Woodstock, Vt.: Plumsock Mesoamerican Studies. 1985. Pp. 334. $14.95.

Despite significant recent work on the nineteenth century, the idea lingers that the premodern society of rural Latin America was set in the long colonial era and emerged only in the mid-twentieth century as the "agrarian problem." This excellent mongraph by J. C. Cambranes helps correct such impressions by exploring in depth Guatemala's development in the nineteenth century into a major coffee producer. The social relations that developed along with coffee owed much to the ideological climate as well as to political and economic changes, specific to the nineteenth century, in the consuming countries of Europe and the United States. In Guatemala the liberal veneer prevalent in most of Latin America complemented such specific characteristics as an Indian peasantry long subjugated by a small Creole elite and German dominance in coffee production and trade.

Although not slighting international and macroeconomic factors, Cambranes focuses on the effect of coffee expansion on the country's Indian majority. For them the penetration of capitalist agriculture into this peripheral unit in the world economy was an unmitigated disaster. Indians not only lost land but also were coerced to work on coffee plantations under conditions closely resembling slavery. Consistent with liberalism, communal lands were privatized and community subsistence lost to commodity production benefiting the local elite and their German partners. Less consistently, labor drafts and vagrancy laws legalized and institutionalized the forced labor systems existing de facto since the Spanish conquest.

A major contribution of this book is its solid base in an impressive amount of documentary material. Most of the Guatemalan government sources are closed to outside researchers, and many of the documents left by German coffee planters and diplomats are now in East Germany, not readily accessible to U.S. and Latin American historians. The inclusion of many long quotations from sources may seem cumbersome but is justified because the originals are worthwhile and relatively inaccessible.

Works on Latin America that take the perspective of class analysis in order to recover the memory of the oppressed masses are often criticized for sensationalist subjectivity, slighting evidence for theory, or both. This book, in contrast, makes its case in a way that the most reserved and "objective" historian will find compelling.

THOMAS H. HOLLOWAY
Cornell University

O. NIGEL BOLLAND. *Belize: A New Nation in Central America*. (Westview Profiles: Nations of Contemporary Latin America.) Boulder, Colo.: Westview. 1986. Pp. xiv, 157. $26.50.

This brief volume, like others in this series, is designed to serve as an introduction to a country. In this it resembles the area handbooks and country studies commissioned by the U.S. government. Like those studies, this work is divided topically rather than chronologically. This organization gives it a certain convenience as a reference book, but it also leads to some repetition, for some facts appear in various contexts. After a short introduction the author gives a history of Belize to the 1930s, then discusses its people and cultures, economy, and, lastly, and all too briefly, politics.

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O. Nigel Bolland, a sociologist by training, deals best with the complex cultural groupings of Belize. He points out that the commonly held notion of two cultures, one Creole and Protestant and the other Hispanic and Catholic, is far too simplistic. Cultural attributes, such as language and religion, cut across ethnic and racial lines. He also points out the cultural contributions of such generally neglected groups as the Maya of the interior and the Garífuna (a Carib-black group similar to the Miskito) along the coast. If any factor unites this diverse nation, Bolland finds it in Catholicism, which claims 61.7 percent of the population and thus embraces far more than the 33 percent of the population who are Hispanic mestizos.

In discussing the economy, Bolland notes the exploitative attitude of Great Britain as the mother country and in particular of the Belize Estate and Produce Company, which owned millions of acres and greatly influenced the policies of the colonial government. Today, despite the economic dislocations of the early 1980s, the future of the newly independent nation is seen as promising, for Belize is blessed with abundant natural resources and a relatively small, but well-educated, populace. As the author points out, a lot will depend on new Prime Minister Manuel Esquivel and the course he charts for the country.

On the whole, and especially considering the paucity of work done on Belize, this volume is a welcome contribution to our understanding of Central America's newest nation.

THOMAS P. ANDERSON
Eastern Connecticut State University

DAVID TAMARIN. *The Argentine Labor Movement, 1930–1945: A Study in the Origins of Peronism.* Albuquerque: University of New Mexico Press. 1985. Pp. xiv, 273. $27.50.

Studies of the Argentine labor movement's contribution to the growth of Peronism have dwelt mostly on the brief period between October 1943, as Perón became labor secretary, and October 1945, when the great workers' march on Buenos Aires saved Perón from political extinction. David Tamarin's book stands among the few recent works that have looked at the rise of Peronism from the perspective of labor's development since the late nineteenth century. If the book deals mainly with the period after 1930, Tamarin also includes material and interpretation on earlier years. His study is well organized, comprehensive, original, and, for the most part, persuasive; it stands out as the best and richest analysis of its subject.

Tamarin's chief theme is the response of labor and leftist groups to the economic and social changes of the 1930s that accompanied the industrial development induced by the depression. He contends that during this period labor experienced a transition in "consciousness" that made possible its emergence as the backbone of Peronist populism; Perón himself exemplified a "transition in labor's self-perception and structure" that had occurred before 1943 (p. xiii). Tamarin adduces evidence to show that, after 1930, unions began to support tariff protection to promote employment and state-sponsored social reform to expand the domestic market; labor itself therefore tended to support "class harmony" under state tutelage, which Peronism came to embody.

The argument seems to me a trifle overstated. Although labor wanted state support and patronage, its goal was not Perón's brand of authoritarian "state syndicalism" but welfare social democracy, a system that imitated the New Deal or Clement Atlee's policy of the late 1940s rather than Mussolini's corporatism. Equally, labor's role as Peronism's "columna vertebral" emerged more as an imposition from above than as part of the process of structural change from below: Perón fashioned the labor movement, rather than the labor movement Perón; labor throughout remained a dependent variable and not the central actor that Tamarin's analysis implies.

This book contains an abundance of new research and many sound interpretations. It is extremely well written, if perhaps a little unduly marred by printing errors. Tamarin offers new evidence on industrial employers' support for protection during the early 1930s, and he provides the best account of social conditions for this period. He offers innovative accounts of the histories of key trade unions, such as the railwaymen's Unión Ferroviaria and the construction workers' Federación Obrera Nacional de la Construcción. He clearly illustrates the distinctive strategies of competing factions among the unions and links them with the growth and changing structure of the urban working class. Tamarin radically revises unemployment rates during the depression, suggesting that unemployment was much higher than commonly thought. He offers fascinating new insights on hitherto-shadowy figures such as José Figuerola, a leading architect of Perón's labor enactments through whom the influences of Mussolini and Primo de Rivera on Peronism become more apparent. Tamarin's careful analysis of the events of October 9 to 17, 1945, sheds new light on the great workers' rising that signaled the eruption of Peronism as the greatest of the Latin American populist movements.

DAVID ROCK
University of California, Santa Barbara

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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

CLAUDE E. BARFIELD and WILLIAM A. SCHAMBRA, editors. *The Politics of Industrial Policy.* (Competing in a Changing World Economy Project.) Washington, D.C.: American Enterprise Institute for Public Policy Research. 1986. Pp. xii, 344.

WILLIAM J. BARODY, JR., Foreword. AARON WILDAVSKY, Industrial Policies in American Political Cultures. THOMAS K. MCCRAW, Mercantilism and the Market: Antecedents of American Industrial Policy. ELLIS W. HAWLEY, "Industrial Policy" in the 1920s and 1930s. JAMES FALLOWS *et al.*, Commentaries. STEPHEN S. COHEN *et al.*, Institutions, Politics, and Industrial Policy in France. JEFFREY A. HART, British Industrial Policy. JEFFREY A. HART, West German Industrial Policy. CHALMERS JOHNSON, The Institutional Foundations of Japanese Industrial Policy. MARIO SCHIMBERNI, State-owned and Private Enterprises in Italy. ROBERT PASTOR and HOWARD J. WIARDA, Commentaries. MANCUR OLSON, Supply-Side Economics, Industrial Policy, and Rational Ignorance. SIDNEY BLUMENTHAL *et al.*, Commentaries. HUGH HECLO, Industrial Policy and the Executive Capacities of Government. ROBERT W. RUSSELL, Congress and the Proposed Industrial Policy Structures. STUART E. EIZENSTAT *et al.*, Commentaries.

PAUL E. STEPANSKY, editor. *Freud: Appraisals and Reappraisals.* (Contributions to Freud Studies, number 1.) Hillsdale, N.J.: Analytic. 1986. Pp. xix, 267. $24.95.

PETER J. SWALES, Freud, His Teacher, and the Birth of Psychoanalysis. EDWIN R. WALLACE IV, Freud as Ethicist. BARRY SILVERSTEIN, "Now Comes a Sad Story": Freud's Lost Metapsychological Papers. PATRICK J. MAHONY, The Oral Tradition, Freud, and Psychoanalytic Writing. PAUL E. STEPANSKY, Feuerbach and Jung as Religious Critics—With a Note on Freud's Psychology of Religion. JOHN E. GEDO, On the Origins of the Theban Plague: Assessments of Sigmund Freud's Character.

CHRISTIANE HARZIG and DIRK HOERDER, editors. *The Press of Labor Migrants in Europe and North America, 1880s to 1930s.* (Publications of the Labor Newspaper Preservation Project.) Bremen: Universität Bremen. 1985. Pp. 602. DM 15.

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REVIEW OF BOOKS

TO THE EDITOR:

In his review of our book, *Controlling the Atom: The Beginnings of Nuclear Regulation, 1946–1962* (*AHR*, 91 [1986]: 1019), William Wiecek indicts us for writing a narrow bureaucratic history of the regulatory program of the Atomic Energy Commission. He also charges that we have turned "a blind eye to the AEC's mendacity, blunders, and cover-ups." We cannot let those allegations go unchallenged.

Wiecek contends that we made significant omissions in our book and submits a long list of subjects that he says are essential for understanding the history of nuclear regulation. We did not discuss most of the items and people on his list—ranging from Julius and Ethel Rosenberg to the House Committee on Un-American Activities—simply because they had nothing to do with nuclear regulation in the United States. Wiecek surely knows this; his co-authored book, *Nuclear America: Military and Civilian Nuclear Power in the United States, 1940–1980* (1984) makes no connections between the topics he cites and regulatory matters. In other instances, Wiecek claims that we ignored topics that in fact we did discuss at some length.

Wiecek uses a large part of his allocated space to list omissions and neglects to inform readers about what *is* in the book. *Controlling the Atom* discusses topics that are central to the development of nuclear regulatory programs and policies—radiation protection standards, nuclear insurance, reactor siting, licensing procedures, radioactive waste disposal, and others. These might seem to be narrow bureaucratic questions to Wiecek, but they are absolutely essential to understanding the development of nuclear regulation. If Wiecek has evidence showing that the items he chides us for ignoring are more significant for our subject than those we do emphasize, we hope he will publish his findings soon for the benefit of all scholars working on the history of nuclear energy.

Our strongest objection to Wiecek's review is his claim that our positions as government historians made us unwilling and/or unable to view our subject critically. "The Washington axiom, 'where you sit is where you stand,'" he intones, "also holds true in historical writing." His charge is unfounded; we did not hesitate to fault the AEC on a variety of issues, as many other reviewers of *Controlling the Atom* have noted. Our criticisms were obviously not as harsh as Wiecek would like, and he is welcome to disagree with our judgments. But to question our professional integrity is wholly unwarranted. *Controlling the Atom*, like any scholarly book by an academic or a government historian, should be judged on what it says and not on where we sit. Wiecek set us up as straw men to blow us away with an *ad hominem* attack.

We would like to point out to scholars interested in the history of nuclear regulation that they do not have to rely on our assessment of the subject. As noted in the preface to *Controlling the Atom*, agency documents cited in our book are open for research. They are even available to Wiecek, and if he wishes to examine relevant primary sources in Washington, we can assure him that he can sit—or stand—anywhere he chooses.

GEORGE T. MAZUZAN
Nuclear Regulatory Commission
J. SAMUEL WALKER
Nuclear Regulatory Commission

PROFESSOR WIECEK REPLIES:

George Mazuzan and Samuel Walker allude to numerous differences of view between me and them on what a history of nuclear regulation should

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encompass. For all these differences save one, I am content to let your readers evaluate the merits for themselves by comparing *Controlling the Atom* with my review and perhaps my book, *Nuclear America*.

On one matter, however, a comment from me is necessary. Nowhere in my review did I question the professional integrity of the authors. I have not the slightest doubt of their integrity, honesty, and competence as historians and as human beings. I explicitly stated in the review that both men are professional historians, and I concluded that they had written a competent, if disappointing, book.

The authors fail to distinguish between criticism of the men and criticism of their book. My review contained the latter, but not the former. *Controlling the Atom* is imbued with the viewpoint and values of the Atomic Energy Commission, the predecessor of the Nuclear Regulatory Commission (the authors' employer). Let me illustrate by reference to the example they selected for discussion. I did indeed fault the book (and continue to do so) as a history of nuclear regulation that contains no reference to the Oppenheimer security hearings. It seems axiomatic to the authors that McCarthyism in all its manifestations had no relation to the AEC's nuclear-promotional and nuclear-regulatory policies. It seems equally axiomatic to me that such a conception of the subject is myopic. But nothing in the expression of my opinion implied that the authors are dishonest or incompetent. I did state that they share the world-view of their subject. I have no doubt, nor did I imply any, that their views are sincerely held and based on reasoned conviction. I neither made nor intended to make any allegation that they are improperly sensitive to the opinions of those who employ them. Can we not criticize ideas and beliefs without personalizing the controversy?

Aside from this, I remain convinced that my review was fair. It would take more space than is available to me here or was allotted to me in the original review to support my evaluations of *Controlling the Atom* in the detail necessary to assure its authors that my criticisms were directed at their ideas and not *ad homines*.

WILLIAM M. WIECEK
Syracuse University

TO THE EDITOR:

In reviewing *Albert Venn Dicey: The Man and His Times* (*AHR*, 91 [1986]: 922), I can well understand why Professor Soffer gave Dicey a wide berth when reviewing a book about jurist John Austin, though I do think that due process, fairness and truth required that she write something specific about my political biography of the former Vinerian Professor at Oxford rather than just dismiss it as a mass of injudicious, undocumented conjectures. The mere mention that I thought that Dicey was behind the plot to name Sir Charles Dilke co-respondent in *Crawford v. Crawford* was enough for her to wash her hands of the whole matter.

Nonetheless, reviewers are supposed to give some indication of a book's content. Soffer should have said something about Dicey's religious, moral and political activities as an Oxford undergraduate, London reviewer and militant politician. Unlike an intellectual, Dicey always used ideas for specific ends, whether he was giving battle to Jowett and Wilberforce about true religion or Gladstone and Balfour about Irish policy. While Dicey's mother *and* father did have a great impact upon his development, he learned to appreciate J. S. Mill as a reviewer for *The Reader* and Bagehot and Freeman as reviewers for *The Nation*. Ultimately, Dicey decided that he was more competent than established political leaders to solve the Irish Question, a commitment which would make an incoherent mess of almost everything he touched.

No sooner had Dicey set in motion a plan to bring to power Home-Rule Governments in both London and Washington than he concluded erroneously that the Parnellite movement was not to be trusted, a surmise which would lead him to deliver a polemic against the Irish party, *The Law of the Constitution*. When this did not stop Home Rule, Dicey decided to ruin Dilke's leadership, to direct the Liberal Unionist Association, and to use forgeries, the basis of *The Times* "Parnellism and Crime," to destroy Parnell. When the Parnell Commission persuaded Dicey that he had been grossly wrong about all this, he wrote *The Verdict*, a Home-Rule pamphlet to rectify what he had largely done. When this failed, Dicey became a Unionist again. While this did have its good side, *e.g.*, the writing of *Law and Opinion*, it helped see to the early resignation of James Bryce as Campbell-Bannerman's Irish Secretary, to the fight over Lloyd George's Budget being doubly politicized, and to the suspension of the Third Home Rule Act until after World War I, a period Dicey used to fashion the Government of Ireland Act.

While all these findings are most disturbing, they are not all based on the same kind of evidence, and, consequently, do not have the same level of certainty. Of course, the true nature of Dicey's lectures given at Oxford and Cambridge can be gleaned by reading what he said on those occasions, not his amended revisions or what his glossators have added to the confusion. In this context, Soffer could have shown how Dicey came to his ideas about sovereignty by a completely different route from Austin, and only used the jurist's ideas to bolster his argument. For those who want to learn more about Dicey as the Home Ruler without Parnell, I would suggest that they read the fine print in *The Verdict*.

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Dicey's role in Bryce's resignation can be seen in their correspondence, his role in the mess over the 1909 Budget from his columns in *The Times*, and his role in the Government of Ireland Act from other documented sources.

This leaves things like the Crawford divorce, and I think that to be fair about the matter Soffer should have mentioned that Dicey is on record not only as having destroyed as many of his crucial papers as possible but also not wanting anyone to know his secret activities. The problem of finding out what he did is compounded by the fact that he often wrote anonymous columns. Moreover, at *The Nation*, the record books for the period 1881–1901 have been conveniently lost, and alleged authorship was penciled in much later. This is all most relevant when one tries to determine who, in London, wrote the note to Crawford about his wife's adultery, and *The Nation* column, "The *Pall Mall Gazette* and the Criminal Law Amendment Bill," on the same day. Haskell's *Index* for *The Nation* claims that Bryce wrote the column, but he wrote to its editor, Godkin, on December 8, 1886, protesting what the column had to say about politically exploiting the issue of underage prostitution. So who was behind the plot to destroy Dilke? Dicey often called persons "fools" who did not understand the Irish Question properly, what the note charged Crawford with being for failing to take action against his wife.

If this rejoinder still does not convince readers of Cosgrove's biography that there is much more here than meets a casual eye, amen.

TROWBRIDGE H. FORD
College of the Holy Cross

Reba Soffer was invited to reply to Trowbridge H. Ford's letter and declined to do so.

THE EDITOR

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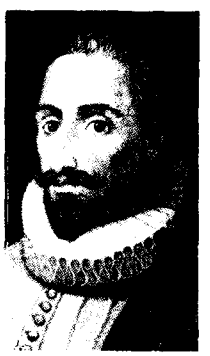


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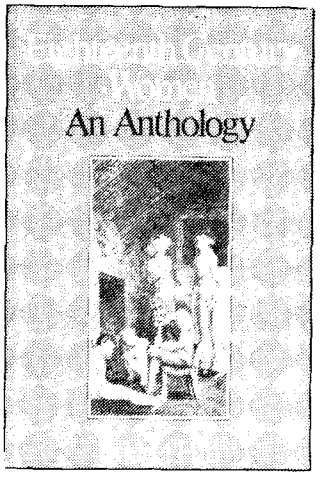
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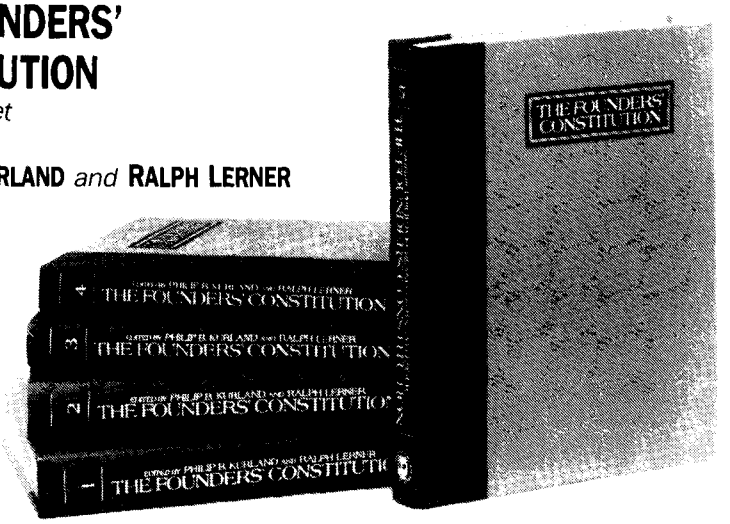
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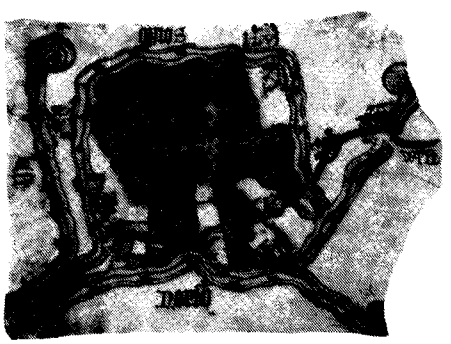


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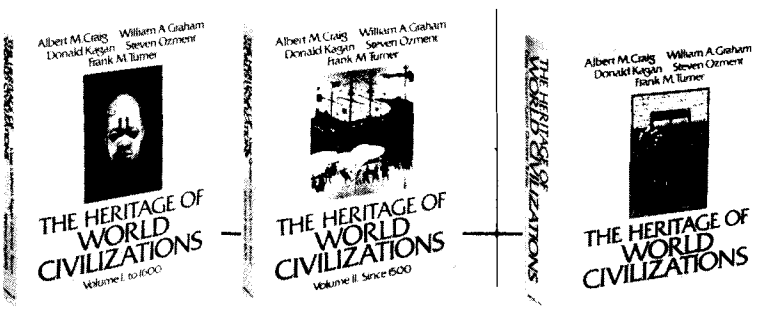
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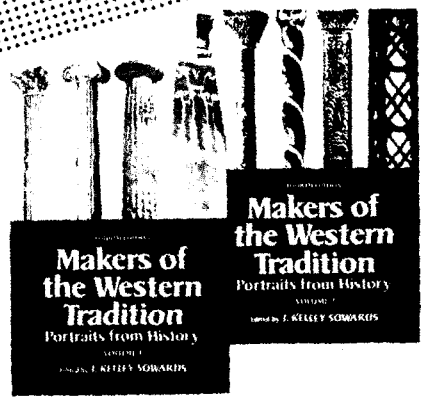
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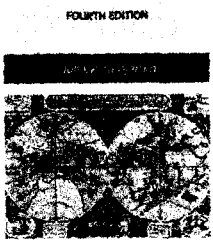


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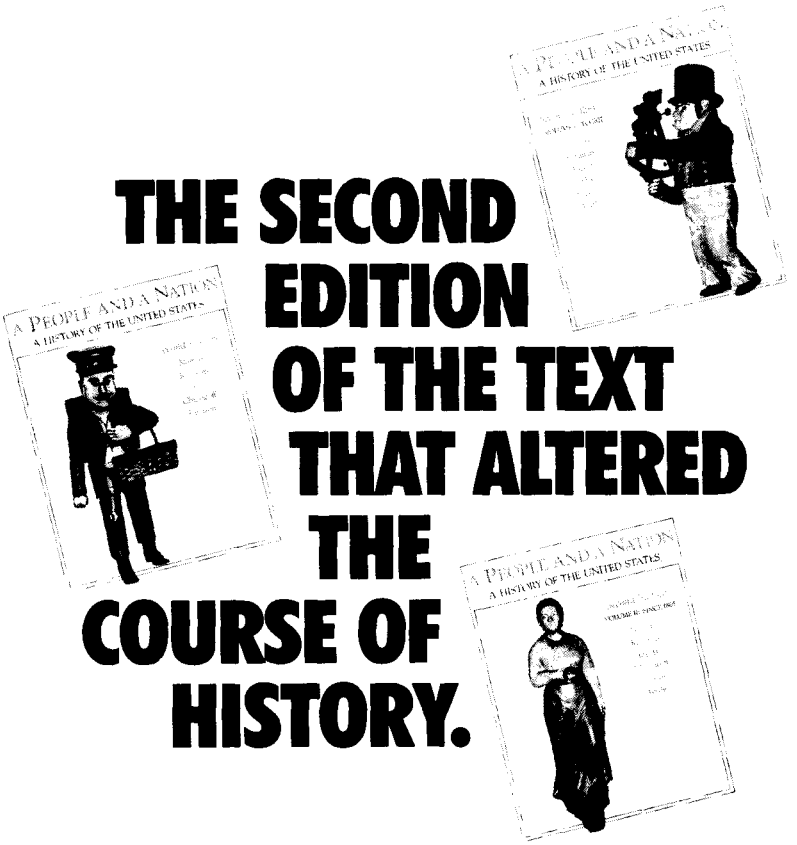
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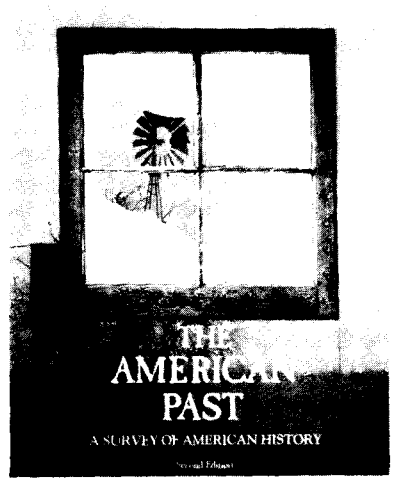
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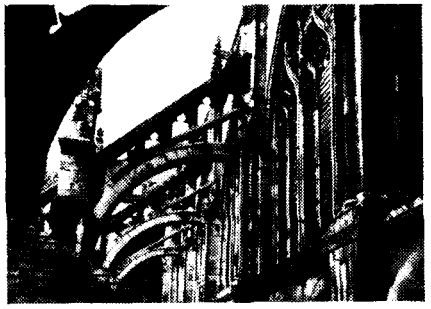
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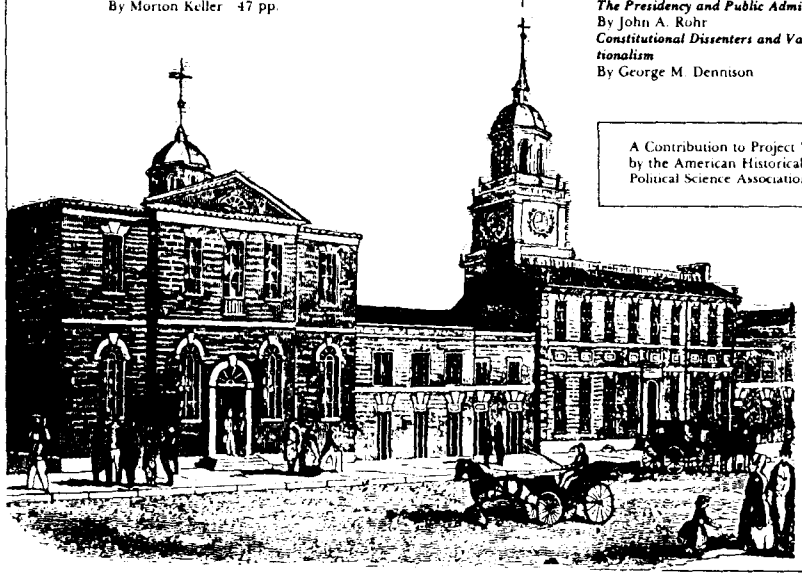
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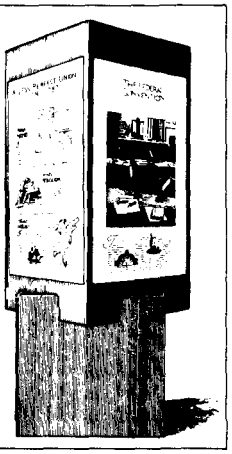
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